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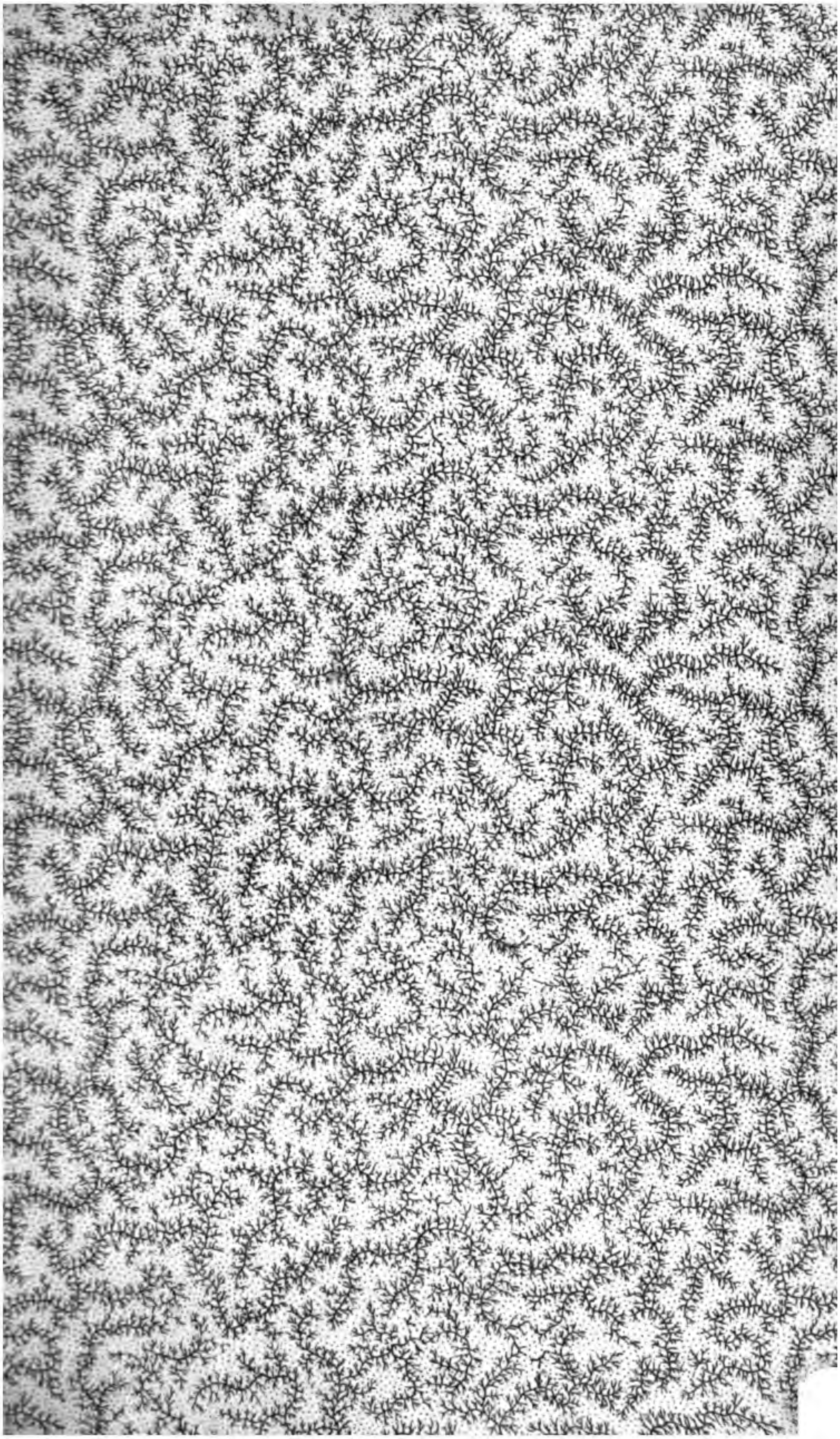

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ARITHMETICAL PRODIGIES.

E. W. SCRIPTURE, PH. D. (Leipzig).

I.

A great deal has been said and written about these phenomenal persons in a very uncritical manner; on the one hand they are regarded as almost supernatural beings, while on the other hand no notice has been taken of them scientifically. Nevertheless, we can perhaps gain light on the normal processes of the human mind by a consideration of such exceptional cases. The first object of the present article is to give a short account of these persons themselves, and to furnish for the first time an approximately complete bibliography of the subject. Thereupon the attempt will be made to make such a psychological analysis of their powers as will help in the comprehension of them, and will perhaps furnish more than one hint to the practical instructor in arithmetic.

NIKOMACHOS.—Lucian said that he did not know how better to praise a reckoner than by saying that he reckoned like Nikomachos, of Gerasa.[1] Whether this refers to the reckoning powers of Nikomachos (about 100 A. D.), or to the famous Introduction to Arithmetic written by him, we are left in doubt. De Morgan inclines to the former opinion,[2] Cantor holds the latter.[3] The literal translation of the passage places Nikomachos undoubtedly among the skillful calculators.

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[1] Lucianus, Philopatris, "ἀριθμέεις ὡς Νικόμαχος."

[2] Smith's Dictionary of Greek and Roman Biography v. Nikomachos.

[3] Cantor, *Vorlesungen über Geschichte der Mathematik*, Leipzig, 1880, I, 363.

AFRICAN SLAVE DEALERS.—Perhaps brought to the front or produced by the necessity of competing with English traders armed with pencil and paper, many of the old-time slave-dealers of Africa seemed to have been ready reckoners, and that, too, for a practical purpose,—a point overlooked by more than one of the later calculators. "It is astonishing with what facility the African brokers reckon up the exchange of European goods for slaves. One of these brokers has perhaps ten slaves to sell, and for each of these he demands ten different articles. He reduces them immediately by the head into bars, coppers, ounces, according to the medium of exchange that prevails in the part of the country in which he resides, and immediately strikes the balance."[1] The ship-captains are said to have complained that it became more and more difficult to make good bargains with such sharp arithmeticians. It was also an African who was the first to appear in this rôle in America.

TOM FULLER.—The first hand evidence in regard to Fuller consists of the following: A letter read before the Pennsylvania Society for the Abolition of Slavery by Dr. Rush of Philadelphia, which is published, more or less completely, in three places;[2] and the obituary which appeared in the Columbian Centinel.[3] On the foundation of these documents several later accounts have been given.[4]

---

[1] [T. Clarkson.] An Essay on the Slavery and Commerce of the Human Species, particularly the African. 2d Ed., London, 1788. (The passage quoted does not appear in the American editions, Phila., 1786, 1787, 1804).

[2] American Museum, Vol. V, 62, Phila., 1799.

Steadman, Narrative of a five years expedition against the Revolted Negroes of Surinam, South America, 2v. 4°, London, 1796, Vol. II, 260. In the French translation, Vol. III, 61.

Needles, Historical Memoir of the Penn. Society for the Abolition of Slavery; Phila., 1848, p. 32.

[3] Columbian Centinel of Boston, Dec. 29, 1790, No. 31 of Vol. XIV.

[4] For example, Grégoire; An Enquiry concerning the Intellectual and Moral Faculties, and Literature of Negroes, followed with an Account of the Life and Works of Fifteen Negroes and Mulattoes; Translated by D. B. Warden; Brooklyn, 1810. (The translation is from Grégoire's original manuscript.) Brissot de Warville; New Travels in the United States of America. performed in 1788; London, 1792, p. 287; 2d Ed., London, 1794, vol. I, 243; Boston, 1797 (reprint of 1st ed.), p. 158; in the original French edition, vol. II, p. 2. Williams; History of the Negro Race in America; New York, 1883, vol. I, 399. Didot's Nouvelle biographie générale v. Fuller.

Thomas Fuller, known as the Virginia Calculator, was stolen from his native Africa at the age of fourteen and sold to a planter. When he was about seventy years old, "two gentlemen, natives of Pennsylvania, viz., William Hartshorne and Samuel Coates, men of probity and respectable characters, having heard, in travelling through the neighborhood in which the slave lived, of his extraordinary powers in arithmetic, sent for him and had their curiosity sufficiently gratified by the answers which he gave to the following questions: First, Upon being asked how many seconds there were in a year and a half, he answered in about two minutes, 47,304,000. Second: On being asked how many seconds a man has lived who is 70 years, 17 days and 12 hours old, he answered in a minute and a half 2,210,500,800. One of the gentlemen who employed himself with his pen in making these calculations told him he was wrong, and that the sum was not so great as he had said—upon which the old man hastily replied: 'top, massa, you forget de leap year. On adding the amount of the seconds of the leap year the amount of the whole in both their sums agreed exactly."[1] Another question was asked and satisfactorily answered. Before two other gentlemen he gave the amount of nine figures multiplied by nine. He began his application to figures by counting ten and proceeded up to one hundred. He then proceeded to count the number of hairs in a cow's tail and the number of grains in a bushel of wheat. Warville says in 1788, "he has had no instruction of any kind, but he calculates with surprising facility."[2] In 1790 he died at the age of 80 years, having never learned to read or write, in spite of his extraordinary power of calculation.[3]

JEDEDIAH BUXTON.—Jedediah Buxton[4] was born in 1702, at Elmton, in Derbyshire, England, where he died in 1772.[5]

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[1] American Museum, V, 62.

[2] Warville, New Travels, p. 158.

[3] Columbian Centinel, loc. cit.

[4] Gentleman's Magazine, 1751, Vol. XXI, p. 61, 347; 1753, vol. XXIII, p. 557; 1754, vol. XXIV, p. 251, which are the original authorities. Chalmer's General Biogr. Dictionary, London, 1812, v. Buxton; Rose, New General Biogr. Dictionary, London, 1848, v. Buxton; Didot's Nouvelle biographie générale, v. Buxton; Michaud's Biographie universelle v. Buxton.

[5] The dates are given on the authority of Lyson's Magna Britannia, London, 1817, vol. V, Derbyshire, p. 157.

Although his father was schoolmaster of the parish and his grandfather had been the vicar, his education was by some chance so neglected that he was not able to scrawl his own name.[1] All his attainments were the result of his own pure industry; the only help he had was the learning of the multiplication table in his youth; "his mind was only stored with a few constants which facilitated his calculations; such as the number of minutes in a year, and of hair's-breadths in a mile."[2] He labored hard with his spade to support a family,[3] but seems to have shown not even usual intelligence in regard to ordinary matters of life. The testimony as to his arithmetical powers is given by two witnesses. George Saxe says: "I proposed to him the following random question: In a body whose three sides are 23,145,789 yards, 5,642,732 yards, and 54,965 yards, how many cubical $\frac{1}{8}$ths of an inch? After once naming the several figures distinctly, one after another, in order to assure himself of the several dimensions and fix them in his mind, without more ado he fell to work amidst more than 100 of his fellow-laborers, and after leaving him about five hours, on some necessary concerns (in which time I calculated it with my pen) at my return, he told me he was ready: Upon which, taking out my pocket-book and pencil, to note down his answer, he asked which end I would begin at, for he would direct me either way. . . . I chose the regular method, . . . and in a line of twenty-eight figures, he made no hesitation nor the least mistake."[4] "He will stride over a piece of land or a field, and tell you the contents of it, almost as exact as if you measured it by the chain. . . . He measured in this manner the whole lordship of Elmton, of some thousand acres, . . . and brought the contents, not only in acres, roods and perches, but even in square inches; . . . for his own amusement he reduced them to square hairs-breadths, computing (I think) 48 to each side of the inch."[1] Various other problems were solved by him with like facility on later occasions, before a different witness.[2]

---

[1] "His total want of education has been attributed to his excessive stupidity when a child, and an invincible unwillingness to learn anything." Lyson's Magna Britannia, V, 157, note.

[2] Journey Book of Engl., Derbyshire, p. 79.

[3] "A day-labourer," Lyson's Magna Britannia, loc. cit. "Either a small land-owner or a day-labourer; but probably the former," The Journey-Book of England, Derbyshire; London, 1841, p. 79.

[4] Gentleman's Magazine, XXI, 61.

From May 17 to June 16, 1725, he was (to use his own expression) drunk with reckoning, by which a kind of stupefaction was probably meant. The cause was the effort to answer the following question: In 202,680,000,360 cubic miles how many barley-corns, vetches, peas, wheat, oats, rye, beans, lintels, and how many hairs, each an inch long, would fill that space, reckoning 48 hairs in breadth to an inch on the flat? His table of measures, which he founded on experiment, used in answering this was:

200 Barley Corns,
300 Wheat Corns,
512 Rye Corns,
180 Oats,
40 Peas,
25 Beans,
80 Vetches,
100 Lintels,
2304 Hairs 1 inch long,
are contained in one solid inch.[3]

Quite curious is Buxton's notation for higher numbers. His system is: Units, thousands, millions, thousands of millions, millions of millions, thousand millions of millions, tribes, thousands of tribes, etc., to thousand millions of millions of tribes; cramps, thousands of cramps, etc., to thousand million of million of cramps; tribes of cramps, etc. to tribes of tribes of cramps.

In regard to subjects outside of arithmetic, his mind seemed to have retained fewer ideas than that of a boy ten years old. On his return from a sermon he never brought away one sentence, having been busied in dividing some time or some space into the smallest known parts. He visited London in 1754, and was tested by the Royal Society. On this visit he was taken to see King Richard III performed at Drury Lane playhouse, but his mind was employed as at church. During the dance he fixed his attention upon the number of steps; he attended to Mr. Garrick only to count the words that he uttered.[1] At the conclusion of the play they asked him how he liked it. He replied "such an actor went in and out so many times and spoke so many words; another so many, etc."[2] He returned to his village and died poor and ignored.

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[1] Gentleman's Magazine, XXI. 61.
[2] Gentleman's Magazine, XXIII, 557, XXIV, 251.
Gent. Mag., XXI, 348.

AMPÈRE.—The first talent shown by André Marie Ampère,[3] *1775, at Lyon, †1836, at Marseilles, was for arithmetic. While still a child, knowing nothing of figures, he was seen to carry on long calculations by means of pebbles. To illustrate to what an extraordinary degree the love of calculation had seized upon the child, it is related that being deprived of his pebbles during a serious illness, he supplied their places with pieces of a biscuit which had been allowed him after three days strict diet.

As soon as he could read he devoured every book that fell into his hands. His father allowed him to follow his own inclination and contented himself with furnishing him the necessary books. History, travels, poetry, romances and philosophy interested him almost equally. His principal study was the encyclopedia in alphabetical order, in twenty volumes folio, each volume separately in its proper order. This colossal work was completely and deeply engraved on his mind. "His mysterious and wonderful memory, however, astonishes me a thousand times less than that force united to flexibility which enables the mind to assimilate without confusion, after reading in alphabetical order matter so astonishingly varied."[4] Half a century afterwards he would repeat with perfect accuracy long passages from the encyclopedia relating to blazonry, falconry, etc.

At the age of eleven years the child had conquered elementary mathematics and had studied the application of algebra to geometry. The parental library was not sufficient to supply him with further books, so his father took him to Lyon, where he was introduced to higher analysis. He learned of himself according to his fancy, and his thought gained in vigor and originality. Mathematics interested him above everything. At eighteen he studied the Mécanique analytique of Lagrange, nearly all of whose calculations he repeated; he said often that he knew at that time as much mathematics as he ever did.

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[1] Gentleman's Magazine, XXIV, 251.

[2] Memoir of Zerah Colburn, p. 174.

[3] Bibliography (for his life), Saint-Beuve, M. Ampère, sa jeunesse, ses études diverses, etc., in the Revue des deux Mondes, 1837, 4 ième série, t. IX, p. 389. M. F. Arago, Eloge d' Ampère (given in a somewhat condensed form by E. Arago, in Michaud's Biogrgraphie universelle, nouv. éd., v. Ampère), translation in Smithsonian Reports, 1872, p. 111. Didot's Nouvelle biographie générale, v. Ampère. Valson, Vie d' Ampère, Lyon, 1886.

[4] Arago, Eulogy on Ampère, Smithsonian Reports, 1872, p. 113; Michaud's Biogr. universelle, I, p. 597.

In 1793 his father was butchered by the revolutionaries, and young Ampère was completely paralyzed by the blow. Rousseau's botanical letters and a chance glance at Horace roused him after more than a year from an almost complete idiocy; and he gave himself up with unrestrained zeal to the study of plants and the Augustan poets. At the age of twenty-one his heart suddenly opened to a new passion and then began the romantic story of his love, which is preserved in his *Amorum* and his letters.[1] Ampère became professor of mathematics, chemistry, writer on probabilities, poet, psychologist, metaphysician, member of the Academy of Sciences of Paris, discoverer of fundamental truths of electrodynamics, and a defender of the unity of structure in organized beings.[2]

Just as he began by learning completely the encyclopedia of the 18th century, he remained encyclopedic all his life, and his last labors were on a plan for a new encyclopedia.

GAUSS.—The arithmetical prodigies might be divided into two classes, the one-sided and the many-sided. The former would include those who like Buxton, Colburn and Dase were mere "reckoning-machines," the other would consist of men in whom the calculating power was only a part of gifts of mathematical talent like Safford, or even of the highest mathematical genius like Gauss.

Carl Friedrich Gauss was born in 1777, in Braunschweig. He was the offspring of a poor family that had in nowise distinguished themselves, although his mother seemed to have been of finer mental build than the paternal stock. Moreover his maternal uncle was a man of unusual talent: completely uninstructed he learned to produce the finest damask; in Gauss's opinion "a natural genius had been lost in him."[1] At an early age the genius of Gauss began to show itself. With the assistance of friends and of persons of the nobility he was enabled to get a school-education. At the age of eleven he entered the gymnasium where he mastered the classical languages with incredible rapidity. In mathematics also he distinguished himself. It is said that a new professor of mathematics handed back thirteen-year-old Gauss's first mathematical exercise with the remark that it was unnecessary for such a mathematician to attend his lessons in the future.[2] The Grand Duke, hearing of his talent, sent for him. The court was entertained by the calculations of the fourteen-year-old boy, but the duke recognized the genius and gave him his support. It is to be regretted that we have not fuller accounts of his early calulations, but his later achievements have so completely occupied the world of science that less attention has been paid to his calculating powers. It is curious to think that if he had had the misfortune to have been gifted with nothing else, he would probably have distinguished himself as Dase or Mondeux did; he might even have proclaimed himself in the Colburn fashion, as a miraculous exception from the rest of mankind; as it is, he was only the greatest mathematician of the century.

---

[1] André Ampère, Correspondence et souvenirs, Paris, 1873.

[2] List of Works in Michaud's Biogr. universelle, nouv. éd., I, p. 611.

After leaving the gymnasium in 1795, he entered the University of Göttingen. As early as 1795, he discovered the method of the least squares, and in 1796 he invented the theory of the division of the circle.

In 1798 he promoted *in absentia* as Dr. phil. at the university of Helmstedt.[3]

In 1801, at the age of twenty-four, his *Disquisitiones arithmeticæ* were published; the work was quickly recognized as one of the milestones in the history of the theory of numbers. From this point on his life was a series of most brilliant discoveries till his death at Göttingen, 1855.

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[1] Hänselmann, K. F. Gauss, Leipzig, 1878, p. 15.

[2] Hänselmann, K. F. Gauss, p. 25.

[3] His dissertation was entitled: *Demonstratio nova theorematis, omnem functionem algebraicum rationabilem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse, Helmstedt, 1798.*

It is much to be regretted that no adequate life of Gauss has yet been written; nevertheless, the story of his discoveries is too well known[1] to need mention. We are here interested in his talent for calculation, for Gauss was not only a mathematical genius,—he was also an arithmetical prodigy, and that, too, at an age much earlier than any of the others.

An anecdote of his early life, told by himself, is as follows: His father was accustomed to pay his workmen at the end of the week, and to add on the pay for overtime, which was reckoned by the hour at a price in proportion to the daily wages. After the master had finished his calculations and was about to pay out the money, the boy, scarce three years old, who had followed unnoticed the acts of his father, raised himself and called out in his childish voice: "Father, the reckoning is wrong, it makes so much," naming a certain number. The calculation was repeated with great attention, and to the astonishment of all it was found to be exactly as the little fellow had said.[2]

At the age of nine Gauss entered the reckoning class of the town school. The teacher gave out an arithmetical series to be added. The words were scarcely spoken when Gauss threw his slate on the table, as was the custom, exclaiming, "There it lies!" The other scholars continue their figuring while the master throws a pitying look on the youngest of the scholars. At the end of the hour the slates were examined; Gauss's had only one number on it, the correct result alone.[3] At the age of ten he was ready to enter upon higher analysis. At fourteen he had become acquainted with the works of Euler and Lagrange, and had grasped the spirit and methods of Newton's Principia.

He was always distinguished for his power of reckoning, and was able to carry on difficult investigations and extensive numerical calculations with incredible ease. His unsurpassed memory for figures set those who met him in astonishment; if he could not answer a problem at once, he stored it up for future solution. At once, or after a very short pause, he was able to give the properties of each of the first couple thousand numbers. In mental calculation he was unsurpassed. He had always in his mind the first decimals of all the logarithms, and used them for approximate estimates while calculating mentally. He would often pursue a calculation for days and weeks, and—what distinguishes him from all other calculators,—during such a calculation he continually invented new methods and new artifices.

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[1] Except to Mr. Sully, who in an article "Genius and Precocity," in the *Nineteenth Century*, never even mentions Pascal, Ampère and Gauss.

[2] Waltershausen; Gauss zum Gedächtniss, Leipzig, 1856, p. 11.

[3] Hänselmann; Karl Friedrich Gauss, Zwölf Kapitel aus seinem Leben, Leipzig, 1878.

Perhaps the best picture of his genius is given by Waltershausen: "Gauss showed a remarkable, perhaps unprecedented, combination of peculiar talents. To his eminent ability to work out in himself abstract investigations on all sides and from all standpoints, there were joined a marvellous power of numerical calculation, a peculiar sense for the quick apprehension of the most complicated relations of numbers, and an especial love for all exact observation of nature."[1]

From Gauss's opinion of Pfaff we get a hint of what he regarded as the essential of genius, "never to leave a matter till he had investigated wherever possible."

WHATELY.—Richard Whately, *1787, Archbishop of Dublin from 1831 to 1863, author of "Historic Doubts relative to Napoleon Bonaparte," "Elements of Logic," "Elements of Rhetoric," and numerous other works, mostly religious, displayed a singular precocity in regard to calculation. At six years old he astonished his family by telling Parkhurst, a man of past sixty, how many minutes he was old.

"There certainly was something peculiar in my calculating faculty," wrote Whately in his Commonplace Book. "It began to show itself between five and six, and lasted about three years. One of the earliest things I can remember is the discovery of the difference between even and odd numbers; . . . I soon got to do the most difficult sums, always in my head, for I knew nothing of figures beyond numeration, nor had I any names for the different processes I employed. But I believe my sums were chiefly in multiplication, division and the rule of three. In this last point I believe I surpassed the famous American boy, though I did not, like him, understand the extraction of roots. I did these sums much quicker than any one could upon paper, and I never remember committing the smallest error." . . .

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[1] Waltershausen, Gauss, p. 83.

"When I went to school, at which time the passion was worn off, I was a perfect dunce at cyphering, and so have continued ever since."[1]

ZERAH COLBURN.[2]—Autobiographies do not always furnish the most trustworthy evidence in regard to the man himself; when, moreover, the author is convinced that he is nothing less than a modern miracle; and, finally, when having had no scientific and little literary education, he at a later date writes the memoirs of his youth, we are obliged to supply the lacking critical treatment of the narrative. The main source of information in regard to Colburn's youthful powers consists of his memoirs published by him in 1833.[3] Only one contemporary account of his earliest exhibitions in America isto be found, we must rely mostly on his own statements, probably derived from recollections of his friends, and on a "Prospectus," a sort of advertisement, published in London in 1813.

Zerah Colburn,* 1804,† 1840, of Cabot, Vt., was considered a very backward child. In the year 1810, a short time after a six weeks attendance at the district school, in which he had learned no arithmetic [unless from the recitations of other boys in the class-room], his father heard him saying "5 times 7 are 35," "6 times 8 are 48," etc., and upon examining him and finding him perfect in the multiplication table, he asked the product of 13 × 97, to which 1261 was instantly given in answer. The account given by Zerah himself, when stated in plain terms, amounts to this; nevertheless, one is tempted to ask for the authority on which the statements were made. If Zerah remembered the exact figures himself till the time of writing his memoirs, then his power of memory for long periods must have been extraordinary, yet he never mentions such powers. On the other hand, if these statements are made from the stories current about him, the general untrustworthiness of such evidence does not allow us to put too much faith in the figures.

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[1] Jane Whately, Life and Correspondence of Richard Whately, D. D., London, 1866, I, 4.

[2] Bibliography; A Memoir of Zerah Colburn, written by himself, Springfield, Mass., 1833; Medical and Philosophical Journal and Review, New York, 1811, Vol. III, p. 19; Philosophical Magazine, XL., London, 1812, p. 119; Philosophical Magazine, XLII, London, 1813, p. 481 [report of the proceedings of the Royal Society, in which a letter concerning the extra digits on members of the Colburn family was read]; Analectic Magazine, Vol. I, year 1813, p. 124 ff. [contains a reprint of an article by the calculator, Mr. Francis Baily, in the Literary Panorama, Oct. 1812, which article is almost identical with the one in the Philosophical Magazine, 1812, p. 119.] Graves; Life of Sir William R. Hamilton, I, 78 ff.; American Almanac, 1840, p. 307. In Faraday's commonplace book there is an unpublished account of Zerah Colburn, who visited Faraday in 1816 and explained to him his method of calculation; Jones, Life of Faraday, London, 1870, I, 221. Gall, Functions of the Brain, Organology, XVIII; R. A. Proctor, On Some Strange Mental Feats, Cornhill Mag., Aug. 1875, Vol. XXXII, p. 157, reprinted in Science Byways, London, 1875, p. 337. [This is an attempt at explaining Colburn's powers; the objections to it will be found below.] R. A. Proctor, Calculating Boys, in Belgravia, Vol. XXXVIII, p. 450 [contains a further explanation.] Carpenter, Mental Physiology, Chap. VI, § 205 [quoting from Baily's account.]

[3] There is no statement regarding the time at which they were written, or even a date to the preface; the last year mentioned in the book is 1827.

Before long Zerah's father took him to Montpelier, Vt., where he was exhibited. Of his performances here Colburn gives only three specimens. "Which is the most, twice twenty-five, or twice five and twenty (2×25 or $2 \times 5 + 20$?) Ans.—Twice twenty-five. Which is the most, six dozen dozen, or half a dozen dozen ($6 \times 12 \times 12$ or 6×12?) Ans.—6 dozen dozen. It is a fact, too, that somebody asked how many black beans would make five white ones? Ans.—5, if you skin them."[1] It is at once apparent that these questions do not demand any extraordinary calculating powers, but on the other hand, a sharpness of wit and an analytical quickness of comprehending puzzles that would be phenomenal in a joker and riddle-maker of ripe years. If it is really true that the child answered the last of these questions, then the real miracle is that he should on not a single other occasion of his life have shown a sign of the Yankee quickness and shrewdness here implied.

On the journey to Boston, Zerah's wonderful gifts convinced A. B., Esq., that "something had happened contrary to the course of nature and far above it;" he was compelled by this "to renounce his Infidel foundation, and ever since has been established in the doctrines of Christianity." At

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[1] Memoirs, p. 12.

Boston he gave public exhibitions. "Questions in multiplication of two or three places of figures, were answered with much greater rapidity than they could be solved on paper. Questions involving an application of this rule, as in Reduction, Rule of Three, and Practice, seemed to be perfectly adapted to his mind." The extraction of the roots of exact squares and cubes was done with very little effort; and what has been considered by the Mathematicians of Europe an operation for which no rule existed, viz., finding the factors of numbers, was performed by him, and in course of time he was able to point out his method of obtaining them. "Questions in Addition, Subtraction and Division were done with less facility, on account of the more complicated and continued effort of the memory [sic.] In regard to the higher branches of Arithmetic, he would observe that he had no rules peculiar to himself; but if the common process was pointed out as laid down in the books, he would carry on the process very readily in his head."[1]

Among the questions answered at Boston were the following:[2] "The number of seconds in 2000 years was required?"

$$\left.\begin{array}{r}\text{730,000 days,}\\ \text{17,520,000 hours,}\\ \text{1,051,200,000 minutes,}\\ \text{63,072,000,000 seconds,}\end{array}\right\}\text{Answer.}$$

"Supposing I have a corn-field, in which are 7 acres, having 17 rows to each acre; 64 hills to each row; 8 ears on a hill, and 150 kernels on an ear; how many kernels on the corn-field? Answer, 9,139,200."

At this time he was a child only six years old, unable to read and ignorant of the name or properties of one figure traced on paper. The exercise of his faculty under such circumstances causes him later to exclaim: "for it ever has been, and still is, as much a matter of astonishment to him as it can be to any other one; God was its author, its object and aim are perhaps still unknown."[3]

Shortly afterward, on a steamboat journey up to Albany, a gentleman taught Zerah the names and the powers of the nine units, of which he had been previously ignorant. In June, 1811, he visited Portsmouth and answered the following: "Admitting the distance between Concord and Boston to be 65 miles, how many steps must I take in going this distance, allowing that I go three feet at a step? The answer, 114,400, was given in ten seconds. "How many seconds in eleven years? Answer, in four seconds, 346,896,000. What sum multiplied by itself will produce 998,001? In less than four seconds, 999."[1]

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[1] Memoirs, p. 15.

[2] P. 171, of the Memoirs, perhaps on the authority of the London Prospectus mentioned above, although Colburn does not say so.

[3] Memoirs, p. 15.

Next summer Zerah's father took him to England and made efforts to secure the patronage of the nobility. At a meeting of his friends "he undertook and succeeded in raising the number 8 to the sixteenth power, 281,474,976,710,656. He was then tried as to other numbers, consisting of one figure, all of which he raised as high as the tenth power, with so much facility that the person appointed to take down the results was obliged to enjoin him not to be too rapid. With respect to numbers of two figures, he would raise some of them to the sixth, seventh and eighth power, but not always with equal facility; for the larger the products became the more difficult he found it to proceed. He was asked the square root of 106,929, and before the number could be written down he immediately answered 327. He was then requested to name the cube root of 268,336,125, and with equal facility and promptness he replied 645 [Extracted from a Prospectus printed in London, 1813]."[2]

"It had been asserted . . . . that 4,294,967,297 $(=2^{32} + 1)$ was a prime number. . . . Euler detected the error by discovering that it was equal to $641 \times 6,700,417$. The same number was proposed to this child, who found out the factors by the mere operation of his mind. [Ibid.]"[3]

Colburn is undoubtedly the one referred to as the Russian boy in the Gentleman's Magazine of 1812. He showed himself to the merchants of the London Stock Exchange; one of them gave the boy a guinea of William III, and demanded to know how many years, months and days had elapsed since its coinage; all of which he answered promptly.[1] This is confirmed by a passage in a letter from a friend of S. B. Morse: "Zerah Colburn . . . has called on us. . . . He has excited much astonishment here, and, as they are very unwilling just at this time to allow any cleverness to the Americans, it was said in some of the papers that he was a Russian."[2]

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[1] Memoirs, p. 171.

[2] Memoirs, p. 37.

[3] Memoirs, p. 38. It requires considerable faith to accept this statement, although S. B. Morse met him in London, and a friend of Morse writes that "There was some great arithmetical question, I do not exactly know what, which he solved almost as soon as it was put to him, though it for several years baffled the skill of some of the first professors." Prime, Life of S. B. Morse, New York, 1875, p. 68.

The father and son, after a visit to Ireland and Scotland, returned to London. In 1814 they proceeded to Paris, where the people manifested very little interest in his calculations. This neglect he can only explain by a national defect of character or a crushing historical event. "Whether it were principally owing to the native frivolity and lightness of the French people, or to the painful effect produced by the defeat of their armies and the restoration of the exiled Louis XVIII, cannot be correctly stated; probably it was owing to the former, etc."[3]

He was introduced to and examined by the members of the French "Institute," among whom was La Place. "Three months had now elapsed that he had not been exhibited, but had given his attention to study; even in this short space it was observable that he had lost in the quickness of his computations."[4] Before long his calculating power left him entirely.

By the exertions of Washington Irving, at that time in Paris, the boy obtained admission to the Lyceum Napoleon (or Royal College of Henri IV.) Zerah gives an interesting account of this institution, which was under strict military discipline, and also of Westminster School, in which he was placed on his return to England.

Being in financial straits the father suggests the stage, and so Zerah makes an unsuccessful attempt at acting. Thereafter, in 1821, he starts a private school, which was given up after somewhat more than a year. After his return to America he joined the Congregational church, but soon went over to the Methodists and began to hold religious meetings. He was ordained deacon, and labored thenceforth as an itinerant preacher, till, in 1835, he was appointed "Professor of the Latin, Greek, French and Spanish Languages, and English Classical Literature in the seminary styled the Norwich University."[1] Here he died at the age of 35, leaving a wife and three children.

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[1] Gentleman's Magazine, 1812, Vol. LXXXII, Pt. II, p. 584.
[2] Prime, Life of S. B. Morse, New York, 1875, p. 68.
[3] Memoirs, p. 74.
[4] Memoirs, p. 76.

It is to be remarked that Colburn's calculating powers, such as they were, seemed to have absorbed all his mental energy; he was unable to learn much of anything, and incapable of the exercise of even ordinary intelligence or of any practical application. The only quality for which he was especially distinguished was self-appreciation. He speaks, for example, of Bidder as "the person who approached the nearest to an equality with himself in mental arithmetic."[2] Again, "he thinks it no vanity to consider himself first in the list in the order of time, and probably first in the extent of intellectual power."[3]

Colburn possessed bodily as well as mental peculiarities. His father and great-grandmother had a supernumerary digit on each hand and each foot; Zerah and three (or two?[4]) brothers possessed these extra members, while they were wanting in two brothers and two sisters. These digits are attached to the little fingers and little toes of the hands and feet, each having complete metacarpal and metatarsal bones.[5] Zerah leaves it a matter of doubt "whether this be a proof of direct lineal descent from Philistine blood or not (see 1 Chronicles xx. 6)."[6] A portrait of Colburn was made in Philadelphia in 1810, and placed in the museum,[7] and another was engraved in London in 1812. The origin of the portrait prefixed to his memoirs is not given; it shows a large head, with unusual development of the upper parts; the forehead is rather small and angular, the occiput is small;[1] the eyes are quite large with projecting orbital arch. Gall, who examined the boy without any previous intimation of his character, "readily discovered on the sides of the eyebrows certain protuberances and peculiarities which indicated the presence of a faculty for computation."[2]

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[1] American Almanac, 1840, p. 307, where he is spoken of as Rev. Zerah Colburn. The University of Norwich (Vt.), after a fire in 1866, was removed to Northfield, Vt.

[2] Memoirs, p. 175.

[3] Memoirs, p. 176.

[4] Memoirs, p. 72.

[5] Philos. Mag. XLII, 481.

[6] Memoirs, p. 72.

[7] Memoirs, p. 20.

MANGIAMELE.—In the year 1837 Vito Mangiamele, who gave his age as 10 years and 4 mos., presented himself before Arago in Paris. He was the son of a shepherd of Sicily, who was not able to give his son any instruction. By chance it was discovered that by methods peculiar to himself, he resolved problems that seemed at the first view to require extended mathematical knowledge. In the presence of the Academy Arago proposed the following questions: "What is the cubic root of 3,796,416? In the space of about half a minute the child responded 156, which is correct. What satisfies the condition that its cube plus five times its square is equal to 42 times itself increased by 40? Everybody understands that this is a demand for the root of the equation: $x^3 + 5x^2 - 42x - 40 = 0$. In less than a minute Vito responded that 5 satisfied the condition; which is correct. The third question related to the solution of the equation: $x^5 - 4x - 16779 = 0$. This time the child remained four to five minutes without answering; finally he demanded with some hesitation if 3 would not be the solution desired. The secretary having informed him that he was wrong, Vito, a few moments afterwards, gave the number 7 as the true solution. Having finally been requested to extract the 10th root of 282,475,249, Vito found in a short time that the root is 7."[3] At a later date a committee, composed of Arago, Cauchy and others, complains that "the masters of Mangiamele have always kept secret the methods of calculation which he made use of."[4]

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[1] Medical and Philos. Journal and Rev., N. Y., 1811, Vol. III, p. 21

[2] Memoirs, p. 77.

[3] Comptes rendus des séances de l'Academie des Sciences, 1837, IV, 978.

[4] Comptes rendus, etc., 1840, XI, 952; reprinted in Oeuvres complétes de A. Cauchy, Paris, 1885, I^{re} serie, tome V, p. 493.

Zacharias Dase.[1]—Zacharias Dase (also, Dahse) *1824, †1861, was born with a natural talent for reckoning; in his own opinion his early instruction had very little influence on him; but his powers were later developed by practice and industry.[2] He spent most of his life in Hamburg, but made many journeys through Germany, Denmark and England, giving exhibitions in ready reckoning in the most important towns. He became acquainted with many learned men, among whom were Gauss, Schumacher, Petersen, Encke, et al. On one occasion Petersen tried in vain for six weeks to get the first elements of mathematics into his head. Schumacher credits him with extreme stupidity.

In 1840, Dase exhibited in Vienna. He attended the lectures of Prof. Strasznicky on the elements of mathematics, who seems to have brought him to such a point that under the guidance of a good mathematician he could do scientific work. He was induced to reckon out the value of π, which he did in two months with the formula $\frac{1}{4}\pi = \text{arc tang}\,\frac{1}{2} + \text{arc tang}\,\frac{1}{8} + \text{arc tang}\,\frac{1}{5}$. The result, which is published in Crelle's Journal (loc. cit.), agreed with that of Thibaut. In 1844, he had a position in the Railroad Department at Vienna; in 1845 he appears in Mannheim; in 1846 he seems to have had a position in Berlin.

Dase was ambitious to make some use of his powers in the service of science. In 1847 he had reckoned out the natural logarithms (7 places) of the numbers from 1 to 1,005,000, and was seeking a publisher.[3] In reckoning on paper he possessed all the accuracy of mental calculation, and added to this an incredible rapidity in doing long problems. In the same year he had completed the calculations for the compensation of the Prussian triangulations. In 1850 the largest hyperbolic table, as regards range, was published by him at Vienna, under the title, "*Tafel der natürlichen Logarithmen der Zahlen*"; the same was reprinted in the annals of the Vienna observatory.[1]

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[1] Bibliography: Briefwechsel zwischen Gauss und Schumacher, herausg. von Peters, Altona, 1861. III, 382; V, 30, 32, 277, 278, 295, 296, 297, 298, 300, 301, 302, 303, 304; VI, 27, 28, 78, 112. Crelle; Journal für Mathematik, 1844, vol. XXVII, p. 198. Dase; Factoren-Tafel für alle Zahlen der siebenten Million, Hamburg, 1862; (the introduction contains remarks on Dase and a letter from Gauss.) Schröder's Lexikon der hamburgischen Schriftsteller, Hamburg, 1851, v. Dase. Auszug aus dem Album des Zacharias Dase, Wien, 1850. Anhang dazu, Hamburg, 1850. Preyer, Counting Unconsciously, in Pop. Science Monthly, XXIX, p. 221; reprinted from Die Gartenlaube, 1886, Bd. I. Littell's Living Age, 1857, LIV, p. 61, "On Mental Calculation"; reprinted from the Atheneum. Accounts of Dase are given in two periodicals at present not accessible to me: Allgemeine Literatur-Zeitung, 1861; Zeitschrift für österreichische Gymnasien, vol. XII.

[2] Schröder's Lexikon gives the account of Dase "nach Selbstbericht."

[3] Briefwechsel zw. Gauss und Schumacher, V, 277.

In 1850 Dase went to England to earn money by exhibitions of his talents. Much the same is related of his great powers as in Germany; his general obtuseness also occasioned remark. He could not be made to have the least idea of a proposition in Euclid. Of any language but his own he could never master a word.

In 1849 Dase had wished to make tables of factors and prime numbers from the 7th to the 10th million. The Academy of Sciences at Hamburg was ready to grant him support, provided Gauss considered the work useful. Gauss writes him: "With small numbers, everybody that possesses any readiness in reckoning, sees the answer to such a question [the divisibility of a number] at once directly, for greater numbers with more or less trouble; this trouble grows in an increasing relation as the numbers grow, till even a practiced reckoner requires hours, yes days, for a single number; for still greater numbers, the solution by special calculation is entirely impracticable. . . . You possess many of the requisite qualities [for establishing tables of factors] in a special degree, a remarkable agility and quickness in handling arithmetical operations, . . . and an invulnerable persistence and perseverance."[2] The assistance was granted and Dase gave himself up to the execution of the task. Up to his death, in 1861, he had completed the 7th million and also the 8th, with the exception of a small portion. Thus he was able to turn his only mental ability to the service of science, forming a contrast to Colburn and Mondeux, who enjoyed even greater advantages yet failed to yield any results.

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[1] Tafel der natürlichen Logarithmen, in Annalen der K. K. Sternwarte in Wien, Theil 34, neuer Folge Bd. XIV, Wien, 1851.

[2] Gauss's letter is given in the preface to Dase's Factoren-Tafeln, 7te Million, Hamburg, 1862.

He multiplied and divided large numbers in his head, but when the numbers were very large he required considerable time. Schumacher once gave him the numbers 79,532,853 and 93,758,479 to be multiplied. From the moment in which they were given to the moment when he had written down the answer, which he had reckoned out in his head, there elapsed 54 seconds.[1] He multiplied mentally two numbers each of 20 figures in 6 minutes; 40 figures in 40 minutes; and 100 figures in 8¾ hours, which last calculation must have made his exhibitions somewhat tiresome to the onlookers. He extracted mentally the square root of a number of 100 figures in 52 minutes.

It is curious that although Dase generally reckoned with astonishing accuracy, yet on at least two occasions his powers failed him. While he was in Hamburg, in 1840, he gave striking proofs of his talents, but at times made great mistakes, which luckily for him happened seldomer than his correct answers.[2] In 1845, Schumacher writes, "at a test which he was to undergo before me, he reckoned wrongly every time." This was explained as coming from a headache.

He had one ability not present to such a great degree in the other ready reckoners. He could distinguish some thirty objects of a similar nature in a single moment as easily as other people can recognize three or four. The rapidity with which he would name the number of sheep in a herd, of books in a book-case, of window-panes in a large house, was even more remarkable than the accuracy of his mental calculations.

PROLONGEAU.—A committee of the Academy of Sciences of Paris, including Arago and Cauchy, undertook in 1845 to investigate the powers of a child of 6½ years, who possessed an extraordinary aptitude for calculation. "He solves mentally with great facility problems relating to the ordinary operations of arithmetic and to the solution of equations of the first degree."[3]

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[1] Briefwechsel zw. Gauss und Schumacher, V, 302.

[2] Briefwechsel zw. Gauss und Schumacher, III, 382.

[3] Comptes rendus des séances de l'Academie des Sciences, 1845, t. XX, p. 1629.

GRANDMANGE.—In 1852 the attention of the Academy of Sciences of Paris was called to a young man of 16 years, C. Grandmange, born without legs or arms, who performed mentally very complicated calculations and solved difficult problems.[1] The committee appointed to investigate the case seems never to have reported.

MONDEUX.—Henri Mondeux,[2] *1826, †1862, was the son of a poor wood-cutter in the neighborhood of Tours. Sent at the age of seven to keep sheep, and deprived of all instruction, he amused himself in counting and arranging pebbles. At this period of life pebbles seem to have been his signs for numbers, for he was ignorant of figures. He learned to execute arithmetical operations mentally and to create for his own use ingenious methods of simplification. After long exercise at this calculation, he used to offer to persons he met to solve certain problems such as to tell how many hours or minutes were contained in the number of years which expressed their ages. This awakened the interest of M. Jacoby, a schoolmaster at Tours, who sought him out. Jacoby proposed several problems and received immediate answers and, finding that the boy could neither read, write nor cipher, and that he had no acquaintance with fractions or any of the ordinary rules of arithmetic, he offered to instruct him. Unfortunately the mind that could carry so many figures could not remember a name or an address, so the boy spent a month searching the city before he found his benefactor. He received instruction in calculation and was often shown in neighboring colleges and schools.

Although in other matters he showed only mediocre intelligence, yet he was something more than a mere calculating machine, as is shown for example in his way of solving the following problem: "In a public square there is a fountain containing an unknown quantity of water; around it stands a group of people with vessels capable of containing a certain unknown quantity. They draw at the following rate: The first takes 100 quarts and $\frac{1}{13}$ of the remainder; the second 200 quarts and $\frac{1}{13}$ of the remainder; the third 300 quarts and $\frac{1}{13}$, and so on until the fountain was emptied. How many quarts were there? In a few seconds he gave the answer, and this is the simple process by which he obtained it: Take the denominator of the fraction, substract one; that gives the number of persons. Multiply that by the number of quarts taken by the first person—that is, by 100—and you get the equal quantities taken by each; square this number and multiply by the number of quarts, and you get the quantity in the fountain."[1]

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[1] Comptes rendus, etc., 1852, t. XXXIV, p. 371.

[2] Bibliography: Comptes rendus des séances de l'Academie des Sciences, XI, p. 820, 952. Oeuvres complètes de Cauchy, I$^{\text{re}}$ Série, Paris, 1885, t. V, p. 493, (this is a reprint of Cauchy's report in the Comptes rendus.) Barbier, Vie d'Henri Mondeux, 1841; Jacoby, Biographie d'Henri Mondeux, 1846; Jacoby, la Clef de l'Arithmetique, 1860; Larousse, Dictionaire universelle, v. Mondeux; Didot's Nouvelle biographie générale, v. Mondeux; The Story of a Wonderful Boy Mathematician, in Every Saturday p. 118, vol. XI, June to Dec. 1871; Biographie universelle, Michaud, v. Mondeux.

In 1840, M. Jacoby presented the boy to the Academy of Sciences of Paris. Jacoby had taken note of the processes employed, and the boy was willing to unfold them himself before a commission. On this occasion two questions were given him, one of which was this: "How many minutes in 52 years? The child, who found the problem very simple, responded in a few moments: 52 years of 365 days each, are composed of 27,331,200 minutes, and of 1,639,872,000 seconds."[2] A committee, including Arago and Cauchy, made an exhaustive examination of his powers and reported on the processes used by him. "At present he easily executes in his head not only diverse operations of arithmetic, but also in many cases the numerical resolution of equations: he invents processes, sometimes remarkable, to solve various questions which are ordinarily treated with the aid of algebra."[3] In spite, however, of his marvellous power of inventing and applying arithmetical methods, he did not answer the expectations of his friends, but sank into obscurity and died almost unknown.

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[1] Every Saturday, XI, 118.
[2] Comptes rendus, etc., 1840, t. XI, p. 820.
[3] Comptes rendus, etc., 1840, XI, 953.

George Bidder.—Geo. Bidder,[1] *1806, †1878, was the son of an English stonemason. His first and only instruction in numbers was received at about 6 years of age, from his elder brother, from whom he learned to count up to 10 and then to 100.

"I amused myself," he says, "by repeating the process [of counting up to 100], and found that by stopping at 10, and repeating that every time, I counted up to 100 much quicker than by going straight through the series. I counted up to 10, then to 10 again=20, 3 times 10=30, 4 times 10=40, and so on. This may appear to you a simple process, but I attach the utmost importance to it, because it made me perfectly familiar with numbers up to 100; . . . at this time I did not know one written or printed figure from another, and my knowledge of language was so restricted, that I did not know there was such a word as 'multiply'; but having acquired the power of counting up to 100 by 10 and by 5, I set about, in my own way, to acquire the multiplication table. This I arrived at by getting peas, or marbles, and at last I obtained a treasure in a small bag of shot: I used to arrange them in squares, of 8 on each side, and then on counting them throughout I found that the whole number amounted to 64: by that process I satisfied my mind, not only as a matter of memory, but as a matter of conviction, that 8 times 8 were 64; and that fact once established has remained there undisturbed until this day, . . . in this way I acquired the whole multiplication table up to 10 times 10; beyond which I never went; it was all that I required."[2]

Most of the child's time was spent with an old blacksmith.

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[1] Bibliography: Proceedings of the Institution of Civil Engineers, vol. XV., session 1855-56, London, 1856, p. 251 ff., "On Mental Calculation"; Vol. LVII, session 1878-79, part III, London, 1879, p. 294, "Memoirs of Deceased Members." A Memoir of Zerah Colburn, written by himself, Springfield, 1833, p. 175. Philosophical Magazine, XLVII, London, 1816, p. 314. Reviews of Bidder's speech are given in Littell's Living Age, 1856, XLIX, p. 254; 1857, LIV, p. 61. A correspondent in the Spectator, 1879, LII, p. 111, quotes from a pamphlet in his possession, the title-page of which is missing. The printers of this pamphlet were M. Bryan & Co., Bristol, and the date is estimated to be 1820. It contains a large number of questions proposed to Bidder at various places in the years 1816-19; the answers given are appended, often with the time it took him to perform the operation.

[2] Proceedings Civ. Eng., XV, p. 257.

On one occasion somebody by chance mentioned a sum and the boy astonished the bystanders by giving the answer correctly. "They went on to ask me up to two places of figures, 13 times 17 for instance; that was rather beyond me at the time, but I had been accustomed to reason on figures, and I said 13 times 17 means 10 times 10 plus 10 times 7, plus 10 times 3 and 3 times 7. . . . . ."[1]

While remaining at the forge he received no instruction in arithmetic beyond desultory scraps of information derived from persons who came to test his powers, and who often in doing so gave him new ideas and encouraged the further development of his peculiar faculty, until he obtained a mastery of figures that appeared almost incredible. "By degrees I got on until the multiple arrived at thousands. Then . . . . it was explained to me that 10 hundreds meant 1000. Numeration beyond that point is very simple in its features; 1000 rapidly gets up to 10,000 and 20,000, as it is simply 10 or 20 repeated over again, with thousands at the end, instead of nothing. So by degrees I became familiar with the numeration table, up to a million. From two places of figures I got to three places; then to four places of figures, which took me up of course to tens of millions; then I ventured to five and six places of figures, which I could eventually treat with great facility, and as already mentioned, on one occasion I went through the task of multiplying 12 places of figures by 12 figures, but it was a great and distressing effort."[2]

Before long he was taken about the country by his father for the purpose of exhibition. This was so profitable for the father that the boy's education was entirely neglected. Even at the age of ten he was just learning to write; figures he could not make. Some of the questions he had answered were the following: "Suppose a cistern capable of containing 170 gallons, to receive from one cock 54 gallons, and at the same time to lose by leakage 30 gallons in one minute; in what time will the said cistern be full?" "How many drops are there in a pipe of wine, supposing each cubic inch to contain 4685 drops, each gallon 231 inches and 126 gallons in a pipe?" "In the cube of 36, how many times 15228?"[1] Among others the famous Herschel came in 1817 to see the "Calculating Boy."

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[1] Proceedings Civ. Eng., XV, p. 258.
[2] Proceedings Civ. Eng., XV, p. 259.

Shortly afterward he was sent to school for a while. Later he was privately instructed, and then attended the University of Edinburgh, obtaining the mathematical prize in 1822. Later he entered the Ordnance Survey, and then was employed by the Institution of Civil Engineers. He was engaged in several engineering works of importance; he is also to be regarded as the founder of the London telegraphic system. His greatest work was the construction of the Victoria (London) Docks. Bidder was engaged in most of the great railway contests in Parliament, and was accounted "the best witness that ever entered a committee room." He was a prominent member, Vice President, then President of the Institution of Civil Engineers. In his later years there was no appreciable diminution in Bidder's powers of retaining statistics in his memory and of rapidly dealing with figures. Two days before his death the query was suggested that taking the velocity of light at 190,000 miles per second, and the wave length of the red rays at 36,918 to an inch, how many of its waves must strike the eye in one second. His friend, producing a pencil, was about to calculate the result, when Mr. Bidder said, "You need not work it; the number of vibrations will be 444,433,651,200,000."[2]

The fact that Bidder became a highly educated man, and one of the leading engineers of his time; that his powers increased rather than diminished with age; and above all, that he has given a clear and trustworthy account of how he obtained and exercised his talent, renders his testimony of the highest worth, and provides the solution of many of the dark problems met with in the cases of Dase, Colburn, and others. Indeed, he seems to fill out just what is lacking in each case; Dase never gave a good account of the way in which he worked; Colburn could not till later explain his methods, and then only in the clumsy way to be expected from a young man of little education; finally, just the part we cannot understand in Buxton is here explained in full.

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[1] Philos. Mag. XLVII, p. 315.

[2] Proceedings Civ. Eng., LVII, 309.

In 1814 a witness to his powers states that he displayed great facility in the mental handling of numbers, multiplying readily and correctly two figures by two, but failing in attempting numbers of three figures. This same witness was present at an examination of the boy in 1816 by several Cambridge men. The first question was a sum in simple addition, two rows with twelve figures in each row; the boy gave the correct answer immediately. After more than an hour the question was asked, "Do you remember the sum in addition I gave you?" He repeated the twenty-four figures with only one or two mistakes. At this time he could not explain the processes by which he worked out long and intricate sums. "It is evident that in the course of two years his powers of memory and calculation must have been gradually developed."[1]

This development seems to have been steady. The following series shows the increasing rapidity with which the answers came:

1816 (10 years of age). What is the interest of £4,444 for 4,444 days, at 4½% per annum? Ans. in 2 min., £2,434, 16*s.* 5¼*d.*

1817 (10 years of age). How long would a cistern 1 mile cube be filling, if receiving from a river 120 gallons per minute without intermission? Ans. in 2 minutes—years 14,300, days 285, hours 12, minutes 46.

1818 (11 years of age). Divide 468,592,413,563 by 9,076. Ans. within 1 min., 51,629,838.

1818 (12 years of age). If the pendulum of a clock vibrates the distance of 9¾ inches in a second of time, how many inches will it vibrate in 7 years, 14 days, 2 hours, 1 minute, 56 seconds, each year being 365 days, 5 hours, 48 minutes, 55 seconds? Ans. in less than a minute, 2,165,625,744¾ inches.

1819 (13 years of age). To find a number whose cube less X 19 multiplied by its cube shall be equal to the cube of 6. Ans. instantly, 3.[2]

Sir Wm. Herschel put the following question to the boy:

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[1] Spectator, 1879, LII, p. 47.

[2] Spectator, 1879, LII, p. 111.

Light travels from the sun to the earth in 8 minutes, and the sun being 98,000,000 miles off, if light would take 6 years and 4 months traveling at the same rate from the nearest fixed star, how far is that star from the earth, reckoning 365 days and 6 hours to each year, and 28 days to each month? Ans., 40,633,740,000,000 miles.[1]

Curious enough is the fact that Bidder and Colburn met in Derbyshire, and underwent a comparative examination, the result of which is said to have been to the total defeat of Colburn.[2]

Prof. Elliot, of Liverpool, who knew Bidder from the time they were fellow-students in Edinburgh, says he was a man of first-rate business ability and of rapid and clear insight into what would pay, especially in railway matters. As a proof of this statement we can accept the fact that Bidder became a wealthy man.

The Bidder family seem to have been distinguished for mental traits resembling George Bidder's in some part or another. Bidder was noted for his great mathematical ability and his great memory. One of his brothers was an excellent mathematician and an actuary of the Royal Exchange Life Assurance Office.[3] Rev. Thomas Threlkeld, an elder brother, was a Unitarian minister. He was not remarkable as an arithmetician, but he possessed the Bidder memory and showed the Bidder inclination for figures, but lacked the power of rapid calculation. He could quote almost any text in the Bible, and give chapter and verse.[4] He had long collected all the dates he could, not only of historical persons, but of everybody; to know when a person was born or married was a source of gratification to him.[5]

One of George Bidder's nephews at an early age possessed remarkable mechanical ingenuity.

Most interesting of all is the partial transmission of his peculiar faculties to his son, George Bidder, Q. C., and through him to two grandchildren. The second son was a first-class man in classics at Oxford, and Fellow of his college. The elder Bidder, however, possessed the peculiar faculties of the family is such proportions that he far exceeded the others in calculating powers.

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[1] Spectator, 1879, LII, p. 112.

[2] Spectator, 1879, LII, p. 112.

[3] Spectator, 1878, LI, p. 1635.

[4] Spectator, 1878, LI, p. 1635.

[5] Brierly's Journal, Jan. 25, 1879, quoted in the Spectator, 1879, LII p. 143.

GEORGE BIDDER, Q. C.—Bidder's calculating faculty was transmitted to his eldest son. It has caused some confusion that he bore the same name as his father. Some writers have lately referred to the father as G. P. Bidder, but since he was always known as Geo. Bidder, the only way out of the difficulty is to distinguish the son by adding his title.

George Bidder, Q. C., distinguished himself at Cambridge in mathematics, being seventh wrangler of his year. He is now a thriving barrister and Queen's Counsel.

He possesses a remarkable visual memory. He always sees mental pictures of figures and geometrical diagrams. "If I perform a sum mentally it always proceeds in a visible form in my mind; indeed, I can conceive no other way possible of doing mental arithmetic."[1]

He considers the special aids to mental calculation to be a powerful memory of a peculiar cast, in which figures seem to stereotype themselves without an effort, and an almost inconceivable rapidity of operation. The former he possessed in a high degree; the latter was no doubt congenital, but was developed by incessant practice and by the confidence thereby acquired.

Bidder says: "I myself can perform pretty extensive arithmetical operations mentally, but I cannot pretend to approach even distantly to the rapidity and accuracy with which my father worked. I have occasionally multiplied 15 figures by 15 [figures] in my head, but it takes me a long time and I am liable to occasional errors." Just before writing this he tried the following to see if he could still do it:

378,201,969,513,825
199,631,057,265,413

"I got in my head the answer 75,576,299,427,512,145,197,597,834,725, in which I think you will find four figures out of the 29 are wrong."[2]

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[1] Spectator, 1878, LI, p. 1634.
[2] Spectator, 1878, LI, p. 1635.

We have no account from George Bidder, Q. C., to show whether he performs the operations rapidly or not.

The daughters of Mr. Bidder, Q. C., show more than the average but not extraordinary powers of doing mental arithmetic. To test their calculating powers Prof. Elliot in 1877 asked them, "At what point in the scale do Fahrenheit's thermometer and the centigrade show the same number at the same temperature?" The nature of the two scales had to be explained, but after that they were left to their own resources. The next morning one of the younger ones (about ten years old) said it was at 40 degrees below zero. This is the correct answer; she had worked it out in bed.[1]

Another granddaughter shows great visual memory. On one occasion she remarked, "When I hear anything remarkable read or said to me, I think I see it in print."

SAFFORD.[2]—Truman Henry Safford was born at Royalton, Vt.,[3] in 1836. Even in his earlier years his parents had amused themselves with his power of calculating. When six years of age he told his mother that if he knew how many rods it was round his father's large meadow he could tell the measure in barley-corns; on hearing that it was 1040 rods, he gave, after a few minutes the answer, 617,760, which he had reckoned out in his head. Before his eighth year he had gone to the extent of Colburn's powers. His abilities were won by means of study, and it was observed that he improved rapidly by practice and lost by neglect.

In 1845, Dr. Dewey wrote of him, "He is a regular reasoner on correct and established princibles, taking the easiest and most direct course. As he had Hutton's Mathematics, and wanted some logarithms, his father told me he computed the logarithms from 1 to 60 by the formula given by Hutton, which were afterwards found to be the same in a table of logarithms for the same number of decimals."[1]

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[1] Spectator, 1878, LI, p. 1634.

[2] Bibliography: Appleton's Cyclopedia of American Biography, v. Safford; Chamber's Edinburgh Journal, July-Dec. 1847, Vol. VIII, p. 265, article "Truman Henry Safford," (this is founded on an article in the "Christian Alliance and Family Visitor," of Boston); Littell's Living Age, XVI, p. 82, has copied the article from Chamber's Journal; Leisure Hour, I, p. 540, contains an abstract of the same article.

[3] It is a curious fact that Safford was born within 40 miles of Colburn's birthplace, and 15 of Norwich.

In his return from a little tour, in which he had been introduced to various scientific men, he set about constructing an almanac which was put to press when the author was just 9½ years old. In the following year he calculated four different almanac calendars. While getting up the Cincinnati one he originated a new rule for getting moon-risings and settings, accompanied by a table which saves full one-fourth of the work in casting moon-risings. This rule and the manuscript almanacs are preserved in the Harvard library, as are also his new rules for calculating eclipses. At ten years of age he was carefully examined by Rev. H. W. Adams, with questions prepared beforehand. Adams says: "I had only to read the sum to him once. . . . Let this fact be remembered in connection with some of the long and blind sums I shall hereafter name, and see if it does not show his amazing power of conception and comprehension."[2] The questions given him became continually harder. "What number is that which, being divided by the product of its digits, the quotient is 3; and if 18 be added the digits will be inverted? He flew out of his chair, whirled around, rolled up his eyes and said in about a minute, 24." "What is the entire surface of a regular pyramid whose slant height is 17 feet and the base a pentagon, of which each side is 33.5 feet? In about two minutes after amplifying round the room, as his custom is, he replied 3354.5558. 'How did you do it,' said I. He answered: Multiply 33.5 by 5 and that product by 8.5 and add this product to the product obtained by squaring 33.5, and multiplying the square by the tabular area taken from the table corresponding to a pentagon."

"Multiply in your head 365,365,365,365,365 by 365,365,365,365,365,365. He flew around the room like a top, pulled his pantaloons over the top of his boots, bit his hand, rolled his eyes in their sockets, sometimes smiling and talking, and then seeming to be in agony, until, in not more than one minute, said he, 133,491,850,208,566,925,016,658,299,941,-

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[1] Chamber's Journal, VIII, p. 265.

[2] Chamber's Journal, VIII, p. 266.

583,225! . . . he began to multiply at the left hand and to bring out the answer from left to right."[1]

In the number of figures this exceeds Bidder's longest multiplication, but the repetition of the same figures renders it easier.

Safford had not a one-sided mind; "chemistry, botany, philosophy, geography and history are his sport." "His memory too is very retentive. He has pored over Gregory's Dictionary of the Arts and Sciences so much that I seriously doubt whether there can be a question asked him drawn from either of those immense volumes that he will not answer instantly." This reminds one of the story of Ampère and the encyclopedia.[2]

On an invitation of the Harvard University his father removed to Cambridge and Safford was placed under the charge of Principal Everett and Professor Peirce. At the age of 14 he calculated the elliptic elements of the first comet of 1849. After graduating from Harvard in 1854, he spent several years there in the observatory. Since this time he has made many important astronomical calculations and discoveries, and numerous contributions to the astronomical journals. He is at present Professor of Astronomy in Williams College.

In regard to the divisors of large numbers, Safford seemed to possess the power of recognizing in a few moments what numbers were likely to divide any given large number, and then of testing the matter by actual division with great rapidity.[3]

MISCELLANEOUS.—A boy from St. Poelten was exhibited by Gall in Vienna. He was the son of a blacksmith and had received no more instruction at school than his companions. At nine years of age, when they gave him three numbers each expressed by ten or twelve figures, asking him to add them, then to subtract them two by two, to multiply and then divide them by numbers containing three figures, he would give one look at the numbers and announce the result before it could be obtained by others on paper.[1]

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[1] Chamber's Journal, VIII, p. 266.

[2] See Arago's Eulogy on Ampère, translated in the Smithsonian Report, 1872.

[3] Belgravia, XXXVIII, p. 456.

Gall says that an advocate came to him to complain that his son, aged five years, was occupied exclusively with numbers and calculations, and that it was impossible to fix his attention on anything else.[2]

Devaux, a boy of seven years, had a passion for going to all the fairs, and waiting for the traders at the moment when they had closed their accounts; when they made mistakes in their calculations, it was his greatest pleasure to discover the error.[3]

Mr. Van R. of Utica, U. S. A., at the age of six years distinguished himself by a singular facility for calculating in his head; at eight he entirely lost this faculty, and after that time he could calculate neither better nor faster than any other person. He did not retain the slightest idea of the manner in which he performed his calculations in childhood.[4]

The daughter of Lord Mansfield, seen by Spurzheim at London, when she was 13 years old, almost equaled Colburn; she extracted with great facility the square and cube root of numbers of nine places.[5]

Prof. Elliot tells of a half idiot who was remarkable in his own county district for his powers of calculation. He got him to put down his operations in a few cases on paper; his modes of abbreviation were ingenious.[6]

Huber tells of a blind Swiss who solved the most difficult arithmetical problems, and who was able to repeat in either way a line of 150 figures after hearing them only once.[7]

II.

The duty of a psychological analysis of the powers of arithmetical prodigies would be to determine the processes of which such powers consist and to establish a series of gradations from the normal to the abnormal. It lies, however, outside of our present task to investigate the fundamental arithmetical processes, though just these cases seem to offer a means of clearing up some of the obscurity; we shall not go beyond facts such as, accuracy of memory, arithmetical association, etc., which for our purposes can be regarded as not requiring further analysis.

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[1] Huber; Das Gedächtniss, München, 1878, p. 43.

[2] Gall, Functions of the Brain, Organology, XVIII.

[3] Gall, loc. cit.

[4] Gall, loc. cit.

[5] Medical and Philosophical Journal and Review, New York, 1811, p. 22.

[6] Gall. loc. cit.

[7] Spectator, 1878, LI, p. 1634.

Speaking of the ability to reckon rapidly, Gauss remarks: "Two things must be distinguished here, a powerful memory for figures and a real ability for calculation. These are really two qualities entirely independent of each other, which can be united but are not always so."[1] Bidder's opinion was "that mental calculation depends on two faculties of the mind in simultaneous operation—computing and registering the result."[2] Nevertheless, there are some other important facts in the psychology of the ready reckoners; we shall accordingly consider them in respect to memory, arithmetical association, inclination to mathematics, precocity and imagination.

MEMORY. Perhaps aside from precocity the most remarkable fact in regard to ready reckoners is their power to do long calculations wholly in the mind without making a mistake; next to this would be placed the wonderful rapidity which some of them have shown.

Accuracy of Memory.—The performance of long calculations in the mind depends above all on the accuracy of the memory for a sufficient length of time. For longer periods of time there seems considerable variation among the several calculators, and indeed this power is not an absolute necessity.

Buxton had perhaps the most accurate memory of all. For example, he gave from memory an account of all the ale or strong beer that he had on free cost since he was 12 years of age; this list included 57 different persons and 2130 glasses. "He will leave a long question half wrought and at the end of several months resume it, beginning where he left off, and proceeding regularly till it is completed."[2] Buxton was very slow and clumsy, but extremely accurate in his calculations, a fact which shows that his powers depended on an accurate memory.

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[1] Briefwechsel zwischen Gauss und Schumacher, V, 300.
[2] Proceedings, Civ. Eng., XV, 262.

Much the same is related of Fuller. "Though interrupted in the progress of his calculation and engaged in discourse upon any other subject, his operations were not thereby in the least deranged so as to make it necessary for him to begin again, but he would go on from where he had left off, and could give any or all of the stages through which the calculation had passed."[3]

Of Dase it is related that, "after spending half an hour on fresh questions, if asked to repeat the figures he began with, and what he had done with them, he would go over the whole correctly."[4] Half an hour after using the two numbers mentioned on p. 45, it was asked if he remembered them. "He instantly repeated the two numbers together (as a number containing 25 figures) forwards and backwards; 9 quadrillion, 351 thousand, 738 billions, etc."[5]

Of Colburn we have no account that represents him as having a good memory for a long time, yet he, as well as all the others, must have possessed extensive multiplication tables stored up indelibly in their minds. This is not to be confused with what we ordinarily call accuracy of memory, by which we mean that a thing or a number once seen is always retained. We may, however, extend the term and speak of acquired accuracy, where the retention results from a proper impression on the mind by means of association and repetition. Bidder, and probably several of the others, possessed wonderful memories, especially for figures; the acquisition of such a memory was due to their peculiar training,

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[1] Gent. Mag. XXIII, 557.

[2] Gent. Mag. XXIV, 251.

[3] Columbian Centinel, Dec. 29, 1790, No. 31 of Vol. XIV.

[4] Littell's Living Age, LIV, 1857, p. 62.

[5] Briefwechsel zw. Gauss und Schumacher, V. 302. The notation follows the continental system; in English it would be 9 octillions, 351 septillions, 738 sextillions, etc.

and, we suspect, to a lack of the ordinary mind-killing processes found in our schools. Bidder says: "As regards memory I had in boyhood, at school and at college many opportunities of comparing my powers of memory with those of others, and I am convinced that I do not possess that faculty in a remarkable degree.[1] If, however, I have not any extraordinary amount of memory I admit that my mind has received a degree of cultivation in dealing with figures in a particular manner which has induced in it a peculiar power; I repeat, however, that this power is, I believe, capable of being attained by any one disposed to devote to it the necessary time and attention."[2]

Although an accurate memory for a long time may not be possessed by every rapid calculator, he must be able to retain before the mind with absolute accuracy the results of the various processes performed till he has finished the problem. This we can pre-suppose in the case of every one of the arithmetical prodigies, and indeed it seems to have been the one thing in which Buxton was superior to ordinary mortals.

One secret of such an accurate memory while performing a calculation, lies in relieving it of unnecessary burdens. It will be noticed that the ready-reckoners often divided a multiplier into two factors and multiplied first by one and then the other; *e. g.*, 432 × 56 would be 432 × 8=3456; 432 and 8 can be now forgotten and 3456 × 7=24192; whereas in the ordinary way 432 × 6=2592, must be held in memory, while 432 × 50=21600 is performed, in order that the partial products may be added together.

There are other means used to lighten the work of the memory. Every one of those about whom we know anything in this respect gave his answers and probably did his work from left to right.[3] Colburn's explanation shows how he began with the highest denominations: "the large numbers found first are easily retained because consisting of so many ciphers."[4]

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[1] Proceedings, Civ. Eng. XV, 253.
[2] Proceedings, Civ. Eng. XV, 253.
[3] Memoirs, p. 191.
[4] See Memoirs, p. 189, 190.

Bidder explains why beginning at the left is easier and necessary. "I could neither remember the figures [in the ordinary way of multiplying], nor could I, unless by a great effort, on a particular occasion, recollect a series of lines of figures; but in mental arithmetic you begin at the left hand extremity, and you conclude at the unit, allowing only one fact to be impressed on the mind at a time. You modify that fact every instant as the process goes on; but still the object is to have one fact and one fact only, stored away at one time."[1] In doing the example 373 × 279, "I multiply 200 in 300=60,000; then multiplying 200 into 70, gives 14,000. I then add them together, and obliterating the previous figures from my mind, carry forward 74,600," etc.

"For instance, multiplying 173 × 397, the following process is performed mentally:

100 × 397=39,700
70 × 300=21,000=60,700
70 × 90 . = 6,300=67,000
70 × 7 . . = 490=67,490
3 × 300 . . . = 900=68,390
3 × 90 . . . . = 270=68,660
3 × 7 . . . . . = 21=68,681.

The last result in each operation being alone registered by the memory, all the previous results being consecutively obliterated until a total product is obtained."[2]

In trying to follow the method used by these men we are hampered by our inability to keep the hundreds, thousands, etc., in their proper places. When a person asks you suddenly how many figures in a million, can you answer him instantly? In his instruction for a ready computer De Morgan gives the following rule: "In numeration learn to connect each primary decimal number, 10; 100; 1000, etc., not with the place in which the unit falls, but with the number of ciphers following. Call ten a *one-cipher* number; a hundred a *two-cipher* number; a million a *six-cipher*, and so on."[3]

Various other little helps were used. Bidder reveals some of them: *e. g.*, "in questions involving division of time, distances, weight, money, etc., it is convenient to bear in mind the number of seconds in a year, inches or barley-corns in a mile, ounces and pounds in a cwt. and ton, pence and farthings in a pound sterling, etc. . . . These were always ready for use when they could be applied with advantage. . . . Suppose it is required to find the number of barley-corns in 587 miles, the ordinary process, viz.: $1,760 \times 587 \times 3 \times 12 \times 3 = 111,576,960$, when worked out, requires 56 figures; while, mentally, I should multiply 190,080, the number of barley-corns in a mile, by 587.'"[1] When we consider that certain stock questions continually recur among those answered by the prodigies, the assistance of such facts is apparent. Safford always remembered the divisors of any number he had examined.[2]

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[1] Proceedings, Civ. Eng. XV, 260.
[2] Proceedings, Civ. Eng. XV, 260.
[3] De Morgan, Elements of Arithmetic, London, 1857, p. 161.

Extraordinary as their powers were these men are not the only ones distinguished for remembering numbers. After a whole day's public sale, Hortensius could tell from memory all the things sold and their prices. Niebuhr could dictate a whole column of statistics from memory.[3] It is related that Alex. Gwin at 8 years of age knew the logarithms of all numbers from 1 to 1000. He could repeat them in regular order or otherwise.[4]

Of Dirichlet it is said that he possessed an "extraordinary power of memory, by means of which he had at every moment completely before him what he had previously thought and worked out."[5]

Euler had a prodigious memory for everything; this gave him the power of performing long mathematical operations in his head. While instructing his children, the extraction of roots obliged him to give them numbers which were squares; these he reckoned out in his head. Troubled by insomnia, one night he calculated the first six powers of all the numbers under 20, and recited them several days afterwards.[1]

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[1] Proc. Civ. Eng., XV, 266.

[2] Belgravia, XXXVIII, 456.

[3] Lieber, Reminiscences of Niebuhr, Phila. 1835, p. 46; also, Lieber's Miscellaneous Writings, Phila. 1881, I, 74.

[4] Belgravia, XXXVIII, 462.

[5] Kummer, Gedächtnissrede auf Gustav Peter Lejeune-Dirichlet, Separatabdr. aus dem Abhandl. d. Kgl. Akad. d. Wiss. zu Berlin, 1860, p. 34.

There is on record the case of Daniel McCartney, born 1817, near Mt. Pleasant, Westmoreland Co., Penn., as late as 1871 living in Salem, Columbiana Co., Ohio, who was examined in 1870 by W. D. Henkle, State Commissioner of Public Schools in Ohio. The man showed a remarkable memory. Among other questions put to him were the following which indicate a power not so great as Buxton's but yet remarkable: "Ques.—What is 123 times 456? Ans. (35 seconds), 56,088. Multiply 456 by 100; then 23 by 400; then add; multiply 23 by 56 and add. Ques.—What is 3756 times 182? Ans. (4½ minutes. He became confused), 683,592. Ques.—What is the sum of 26, 67, 43, 38, 54, 62, 87, 65, 53, 44, 77, 33, 84, 56 and 14? (One minute occupied in calling the numbers.) Ans. (Instantly) 803."[2]

Still more remarkable is the case of Wallis, the mathematician. In a letter to Thomas Smith of Madalene College, Wallis tells his own story:

"December 22d, 1669.—In a dark night, in bed, without pen, ink or paper or anything equivalent, I did by memory extract the square root of 30000,00000,00000,00000,00000,00000,00000,00000, which I found to be 1,77205,08075,68077,29353, *ferè*, and did the next day commit it to writing."

"February 18th, 1670.—Joannes Georgius Pelshower (Regimontanus Borussus) giving me a visit, and desiring an example of the like, I did that night propose to myself in the dark without help to my memory a number in 53 places: 2468135791011121411131516182017192122242628302325272931 of which I extracted the square root in 27 places: 157103016871482805817152171 *proximè;* which numbers I did not commit to paper till he gave me another visit, March following, when I did from memory dictate them to him.

Yours, etc., JOHN WALLIS."[3]

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[1] Euleri commentationes arithmeticae collectae, Petropoli, 1849; tomus I, Éloge de L. Euler par N. Fuss, p. XLIX; see also Condorcet's eulogy of Euler.

[2] Remarkable Cases of Memory, in the Journal of Speculative Philosophy, 1871, Vol. V, p. 16.

[3] A copy of this letter is to be found in the Spectator, 1879, Vol. LII, p. 11.

We have here selected a series beginning with Hortensius and Niebuhr, who simply remembered numbers, and proceeding to men who used their memories in calculating with as much success as Buxton. None of these men could well be placed among the arithmetical prodigies, yet Buxton seems to have differed from McCartney only in his interest for figures, whereas in Euler and Wallis the calculating power was lost sight of. Like these men, Buxton showed none of the rapidity seen in all the other calculators.

Performing long calculations in the head has been compared to blindfold chess-playing. When rapidity is left out of consideration, as in Buxton's case, the same power of memory may perhaps account for both. Indeed, Geo. Bidder, Q. C., who possessed a strong power of visual imagery, is able to play two simultaneous games of chess without seeing the board.

Rapidity of Memory.—The rapidity of a memory will depend on the nature of the various processes of the mind which make up the phenomenon called by that name and also on the rapidity with which these processes work. Our power to rapidly commit a group of objects or a line of a dozen figures to memory and to call it up again instantly, depends on the ease and rapidity with which we can impress it on the mind, on the accuracy with which it is retained and the ease and rapidity with which it can be reproduced. The accuracy of retention, being of course only a manifestation of the accuracy of memory, has already been considered.

The ease and rapidity with which a number of objects can be impressed on the memory seem limited in ordinary persons to about five at a glance. Before the days of experimental psychology this was quite a matter of dispute,[1] but it has been in late years definitely settled. The first experiments seem to have been made by Stanley Jevons, who decides that his power does not reach to five with complete accuracy, and that the error in estimating numbers under such conditions, $= \frac{n}{9} - \frac{1}{2}$, where n is the number of objects.[2]

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[1] Some of the opinions are given in Hamilton's Lectures on Metaphysics, Vol. I, 253.

[2] The Power of Numerical Discrimination, in Nature, Vol. III, 281.

Preyer has made some popular experiments, from which he concludes that after practice a person can estimate in general correctly up to nine objects seen for an instant, when these objects are irregularly grouped, and that acquaintance with a symmetric arrangement, as in cards or dominoes, raises the limit to about 40.[1] Cattell also made experiments on the extent of the focus of consciousness, which show that 4 to 5 unconnected impressions (lines, letters, figures) can be simultaneously apperceived. When these elements were placed in well known groups the number rose to 12 and 15.[2]

There are, however, two processes to be distinguished, the perception of the objects and the counting of them. Cattell's experiments show how many can be distinctly apperceived; but the power of counting them may depend on the maintenance of the apperceived and even the perceived objects in the memory for a sufficient time. Wishing to know how it is possible to count a number of objects seen for so brief a time, I exposed a few objects to the view for an instant; the person observing had then to tell how many objects were seen. One of the observers gave the first number thought of without being able to tell why; the other always counted or attempted to count the objects from a picture of them which he held in his memory.

All the arithmetical prodigies possessed a remarkable impressibility; they were able to grasp large numbers of figures on only once seeing or hearing them. Dase, morever, has given special proofs of his power by his experiments in rapid counting. "When you throw a handful of peas on the table, a casual glance is sufficient to enable him to tell you their number. He did the same . . . with the points of dominoes at which he gave only a momentary glance in order to tell their sum (117)."[3] "He counted the letters in a line on an octavo and a quarto page (47 and 63) after a hasty glance."[4] Dase's memory also possessed great impressibility for figures. "Twelve figures being written down . . . he would just dip his eye upon them, not resting on them more than half a second. He would then repeat them backwards and forwards, and name any one at command, as the ninth or the fourth."[1] Dase can be contrasted with ordinary individuals in this respect. The experiments referred to in the AMERICAN JOURNAL OF PSYCHOLOGY, Vol. II, 607, 608, show that the largest number of numerals that could be learned by once hearing them at the rate of 120 to the minute, was 8.6 for boys of 19 years. Even Mondeux required 5 minutes to learn and retain a number of 24 figures divided into 4 periods, in such a way that he could give at will the six figures in each period.

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[1] Counting Unconsciously. in Pop. Science Monthly, XXIX, p. 221.

[2] Cattell, Psychometrische Untersuchungen, in Philos. Studien, III, 121; Wundt, Phys. Psychol. 3 Aufl. II, 247.

[3] Briefwechsel zw. Gauss und Schumacher, V, 277.

[4] Briefwechsel zw. Gauss und Schumacher, V, 302.

Such quick apprehension of a number as Dase's can be explained by great impressibility; in which case the visual image would be in such a short time so firmly and vividly impressed on the memory that he could turn away his eyes and count the peas or domino points from a still persistent image, just as the person mentioned above did. The case would then be exactly like that of Robert Houdin and his son. They would pass rapidly before a toy shop and cast an attentive glance upon it; a few steps further on they tried which could describe the greatest number of objects seen. The son often reached 40, the father 30. An instance is also given in which the son saw at a glance and remembered the titles of many books of a library. This power of memory was not a natural gift. Houdin taught his son by laying dominoes before him; instead of letting him count the points the boy had to tell the total at once. In three days he could count six dominoes (from 15 to 61 points) and in a short time he could give instantaneously the sum of a dozen (up to 106 points.) In like manner it is possible to learn to commit a row of figures to memory in an instant. "A useful faculty, easily developed by practice, is that of retaining a retinal picture. A scene is flashed upon the eye: the memory of it persists, and details, . . . may be studied . . . in a subsequent vision."[2]

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[1] Littell's Liv. Age, 1857, LIV, p. 62.
[2] Galton, Inquiries into Human Faculty, London, 1883; p. 107.

ARITHMETICAL ASSOCIATION. The psychology of calculation is still an unexplored field; yet for our purpose we can regard the association of numbers as elementary, leaving further analysis for future investigation. The process is that which was taught us in school; we learned to say 1 and 1 make 2, 1 and 2 make 3, etc., 1 less 1 leaves 0, 2 less 1 leaves 1, etc., 1 × 1 = 1, 1 × 2 = 2, etc.; 1 divided by 1=1, 2 divided by 1=2, and so on through the rest of the tables. By this means firm associations are gradually established between any two numbers up to 10 (in older boys often to 12 and 15) in all of the four relations. After thoroughly learning these associations we are able to "do sums." Suppose we had this example to solve: What is the sum of 2571 and 4249? The process we go through is—when we write in the order we do it—as follows:

9 and 1 = 10, put down 0, carry 1.
4 and 7 = 11, and 1 = 12, put down 2, carry 1.
2 and 5 = 7, and 1 = 8.
4 and 2 = 6.
total, 6 thousand, 8 hundred and 20.

Or take an example in multiplication, *e. g.*, 136 by 43. What do you say to yourself while working it?

3 times 6 are 18, put down 8, carry 1;
3 times 3 are 9, and 1 are 10, put down 0, carry 1;
3 times 1 are 3, and 1 are 4;
total, 408.
4 times 6 are 24, put down 4, carry 2;
4 times 3 are 12, and 2 are 14, put down 4, carry 1;
4 times 1 are 4, and 1 are 5;
total 544;
8;
4 and 0 are 4;
4 and 4 are 8;
5;
result, 5848.

This is exactly the way in which children in school generally reckon, even when they have no distinct intention of reckoning aloud. I must also confess that although I long since left school, whenever my mind is tired or distracted I have to go through the same process and cannot put into practice the various methods of "cutting off" that have been since learned.

These "cut-offs" are found in all our activities, and consist in part of a train of thoughts or volitions becoming less and less conscious. Movements which regularly follow certain sense-perceptions have the tendency to become automatic, and *to occupy less time*.[1] In like manner it has been shown that a series of ideas can be gone through, although one of them can sink below consciousness without destroying the sequence.[2] In this way our arithmetical associations can be enormously shortened. In the above example we shall totally disregard the time used in recording results mentally, and try only to shorten the associations. From my own experience I can say that in the first place for most of the associations I can reduce the connecting links between the numbers to an extremely small degree of consciousness, with a greater or less saving of time. Instead of saying "plus," "less," "by," etc., I simply repeat the numbers and the results, and although I know perfectly what I am doing, and make no confusion among addition, multiplication, etc., nevertheless these relations do not rise above a very low degree of consciousness: "9, 5, 14," "9, 5, 4," "9, 5, 45," are perfectly distinct and clear, yet I do not think consciously of any of the operations performed. In like manner various connecting links can be cut out; so that for instance, the example given above would for me be reduced to

3, 6, 18, 1, 8;[3]
3, 3, 9, 1, 10, 1, 0;
3, 1, 3, 1, 4;
408;
4, 6, 24, 2, 4;
4, 3, 12, 2, 14, 1, 4;
4, 1, 4, 1, 5;
544
5848[4]

"The act of addition must be made in the mind without assistance; you must not permit yourself to say 4 and 7 are 11, 11 and 7 are 18, etc."[5] "Learn the multiplication table so well as to name the product the instant the factors are seen; that is, until 8 and 7, or 7 and 8 suggest 56 at once, without the necessity of saying, 7 times 8 are 56.'"[1] Of course the saving of time is very great; and yet an educated person can work with just as much, perhaps more accuracy, than in the unabbreviated style.

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[1] See Wundt, Phys. Psych. 3 Aufl. II, 319.

[2] Scripture, Ueber den associativen Verlauf der Vorstellungen; Inaug. Diss., Leipzig, 1891.

[3] In such cases I always imagine the number, *e. g.*, 18, and then take away the 1, leaving the 8, so that the "carrying" occurs before the "putting down."

[4] In mental examples I also add from top to bottom, and in easy cases from left to right.

[5] De Morgan, Elements of Arith., p. 162.

Still another shortening can be made; by making an effort I can do "the carrying unconsciously so that in the above case I would say,

3, 6, 18, 8,

and not think of the 1 until the time for adding it in occurs. As De Morgan remarks, "don't *say* 'carry 3' but *do* it." Moreover it is not absolutely necessary to distinctly mark the end figure of each partial product; these products can be kept in memory and added up afterwards. The above example would be carried out by a person who had good command of such a power in somewhat the following manner (the same figures denote that they were operated upon before they entered full consciousness:)

^{3}, ^{6}, 18;
^{3}, ^{3}, 9, ^{1} 10;
^{3}, ^{1}, 3, ^{1}, 4;
408;
^{4}, ^{6}, 24;
^{4}, ^{3}, 12, ^{2}, 14;
^{4}, ^{1}, 4, ^{1}, 5;
544;
5848.

This shortening can in adding be carried to such an extent that only the results are noticed; *e. g.*, as soon as a person catches a glimpse of 405 and 540 he knows the sum. In the above case the time for associating the numbers with their products has become exceedingly small; as Bidder says most of the time is required for the registration of the results on the memory, and this as was shown above can in exceptional cases be very small.

Finally an enormous shortening can be made if the adding, subtracting, multiplying, etc., can be done *before the numbers themselves come into full consciousness.* Münsterberg has shown that associations made in such a low degree of consciousness require comparatively little time.[1] A few years ago I made the attempt to acquire this ability and after considerable practice I was able on the sight of two figures to add or subtract them before they had attracted my full attention; in other words while they were yet in the field of consciousness they aroused the proper association and the result entered the focus of consciousness first.

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[1] Ibid, p. 163.

We might be tempted to carry the process of "cutting-off" consciousness still further, and to say in just the same manner as on the sight of the figures 136 × 43 the partial products spring at once of themselves into the mind of a mathematician so in exceptional cases these partial products might be added before they became fully conscious, so that nothing but the result appears; a further application of the "cut-off" would bring the final answer to the whole problem instantly into mind. To be sure the testimony of the elder Bidder is against this, but it is only an extension of the principle and seems necessary to explain the difficulty of Colburn in telling how he did his examples. In his early years all he could say was that the problem was given and the answer was almost at once there. It would also help to explain such cases as are furnished by Dase; he had been given the number

935173853927;

Schumacher mentioned 7. "As soon as he heard 7 he repeated the number

6546216977489."[2]

This performance of processes before the factors became fully conscious would show itself in the popping of the answer into the mind before the person has thought out clearly how it was obtained. Upon the involuntary answers the rapid calculator would have to rely; if he stopped to make sure of each step time would be lost; he must always go ahead without a question as to whether he is right or not. The younger Bidder says, "I am certain that unhesitating confidence is half the battle. In mental arithmetic it is most true that he who hesitates is lost."[3]

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[1] Münsterberg, Beiträge zur experimentalen Psychologie, Freiburg i/B 1890, Heft 1.

[2] Briefwechsel zw. Gauss und Schumacher, V 302.

[3] Spectator, 1878, p. 1634.

In still another way it would be possible to save time. It is common practice to extend the multiplication table beyond ten at least to 12 × 12. Here two figures are multiplied by two figures. In like manner it is easy to learn the table of 15 or 25 and with an effort we could undoubtedly learn complete tables of addition, subtraction, multiplication, division up to perhaps 30.[1] In an example like the following we would divide the number into periods of two figures each and operate directly with them:

2419 × 3017
19, 17 323
24, 17 408
30, 17 570
30, 24 720

7,298,123

To get an idea of the wonderful ease and rapidity with which examples can be done in this way make use of a multiplication table reaching to 100 × 100.[2] When moreover no time is lost in turning the leaves of such a table, in running down a column and recording the results on paper, then a person who could hold such a table in his head ought to be able to answer many problems in less time than even Dase required.

It is really not so difficult to obtain such a table of the products of two figures. "I formerly knew an instructor whose scholars of 8 to 12 years of age, for the most part knew the Pythagorean table extended to 100 × 100, and who calculated rapidly in the head the products of two numbers of four figures, in making the multiplication by periods of two figures."[3] Did any of the prodigies possess such a table? Considering their enormous powers of memory it would be almost unexplainable if they did not. Although Bidder asserts that he really had no such table, yet Mondeux actually possessed part of such a table, and I think we can pre-suppose it in the case of Colburn, Buxton and even Dase.

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[1] "In my opinion, all pupils who show a tolerable capacity should slowly commit the products to memory as far as 20 times 20." DeMorgan, Elements of Arithmetic, London, 1857, p. 25.

[2] For example, Waldo's Multiplication and Division Table for Accountants, Computers and Teachers in the Primary Schools; New York, 1880.

[3] Lucas, Le calcul et les maschines à calculer, Assoc. française pour l'avancement des sciences, Paris, 1884, p. 2.

A number of other rules by which the processes of addition, subtraction, etc., can be shortened are given by DeMorgan in his Elements of Arithmetic, Appendix I; also in Companion to the Almanac 1844 and Supplement to the Penny Cyclopædia, article Computation.

There are also little "kinks" put in practice by many people of which the ready reckoners were not slow to avail themselves, *e. g.*, multiplying by two easy numbers and taking the difference instead of multiplying by an awkward number. In regard to the example given above p. 36 Bidder says "I should know at a glance, that

$$\begin{array}{lrl} & 400 \times 173 = & 69{,}200 \\ \text{and then} & 3 \times 173 = & \underline{\quad\;\, 519} \\ \text{the difference being} & & 68{,}681.\text{"}^{1} \end{array}$$

Now that we have some idea of how the mind works in solving arithmetical problems, and of how it shortens the time required, let us see how the prodigies actually worked.

Dase, on a test before Schumacher, divided "each number into two parts, of which one contains the highest figures and three zeros, and the other the three lower figures, reckoned the 4 partial products in his head, and *noted down every time* separately the results with pencil, which he afterwards added mentally."[2] As Gauss said, Dase seems to have depended on his remarkable accuracy of memory and to have possessed powers of calculation which at best were not equal to those of many mathematicians. "When he needs 8¾ hours to multiply two numbers each of 100 figures in his head, this is really a foolish waste of time, for a moderately practised reckoner could do the same on paper in *much* shorter, in less than half the time."[3] Gauss, however, was himself such a wonderful reckoner that judging from the standpoint of an ordinary person, he underestimated Dase's powers.

Buxton was much slower, as is seen from the following: "Admit a field 423 yards long and 383 wide, what was the area? After I had read the figures to him distinctly, he gave me the true product, viz., 162009 yards, in two minutes, for

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[1] Proceedings Civ. Eng. XV 260.
[2] Briefwechsel zw. Gauss und Schumacher, V, 32.
[3] Briefwechsel zw. Gauss und Schumacher, V, 300.

I observed by my watch how long every operation took him."[1] On paper this is easily done in twenty seconds. "Allowing the distance between York and London to be 204 miles, I asked him how many times a coach-wheel turned round in that distance, allowing the wheel's circumference to be six yards! In 13 minutes he answered 59,840 times."[2] On paper this requires 35 seconds. The clumsiness of Buxton's methods is phenomenal. He was required to multiply 456 × 378. This he did as follows:

$456 \times 5 = 2280$, which $\times\, 20 = 45600$;
$45600 \times 3 = 136800$.
$2280 \times 15 = 34200$;
$136800 + 34200 = 171000$;
$456 \times 3 = 1368$;
$171000 + 1368 = 172368$.[3]

When Mondeux had to multiply two entire numbers he often divided them into portions of two figures; he recognized that in a case where the factors are equal the operation is simpler, and the rules used by him for obtaining the product, or rather the power demanded are precisely those given by Newton's binomial formula.[4] That is to say, in an example, 2419 × 3017, he would proceed as we have done above, and in a case like 2419 × 2419 (or 2419^2) or 2419 to any power he worked according to the formulas $x^2 + 2\,xy + y^2$ (*i. e.*, $24^2 \times [2 \times 24 \times 19] + 19^2$), $x^3 + 3\,x^2\,y + 3\,xy^2 + y^3$, etc. "Guided by these rules he could give on the instant the squares and cubes of a multitude of numbers; for example, the square of 1204 or the cube of 1006. As he knows almost by heart the squares of all the entire numbers less than 100, the division of the greater numbers into periods of two figures enables him to obtain their squares more easily."[5]

A partial account of Bidder's method's of multiplication has already been given; here it is necessary only to add a few facts left untouched and an explanation of his ways of extracting roots and finding factors. Most important is the contrast between his multiplication table, understood and made part of himself, and the mechanical associations of most people.

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[1] Gent. Mag. XXI, 347.
[2] Gent. Mag. XXI, 347.
[3] Gent. Mag. XXIV, 251.
[4] Comptes rendus, XI, 953.
[5] Comptes rendus, XI, 954.

"In order to multiply up to 3 places of figures by 3 figures, the number of facts I had to store in my mind was less than what was requisite for the acquisition of the common multiplication table up to 12 times 12. For the latter it is necessary to retain 72 facts; whereas, my multiplication up to 10 times 10 required only 50 facts. Then I had only to recollect, in addition, the permutations among the numbers up to a million, that is to say, I had to recollect that 100 times 100 were 10,000; 10 times 10,000 were 100,000, and that ten hundred thousands made a million. . . . . Therefore, all the machinery requisite to multiply up to 3 places of figures was restricted to 68 facts. . . . . If you ask a boy abruptly, "what is 900 times 80," he hesitates and cannot answer, because the permutations are not apparent to him; but if he had the required facts as much at his command as he had any fact in the ordinary multiplication table, viz., that 10 times 10 = 100, and that 900 times 80 was nothing more than 9 times 8 by 100 times 10, he would answer off hand 72,000; and if he could answer that, he would easily say 900 times 800 = 720,000. If the facts were stored away in his mind so as to be available at the instant he would give the answer without hesitation. If a boy had that power at his command he might at once with an ordinary memory proceed to compute and calculate 3 places of figures."[1]

The following gives an insight into the rapidity of Bidder's associations: "Suppose I had to multiply 89 by 73, I should say instantly 6,497; if I read the figures written out before me I could not express a result more correctly or more rapidly; this facility has, however, tended to deceive me, for I fancied that I possessed a multiplication table up to 100 times 100, and when in full practice even beyond that; but I was in error; the fact is that I go through the entire operation of the computation in that short interval of time which it takes me to announce the result." The velocity of the mental processes cannot be adequately expressed; the utterance of words cannot equal it. . . . . Were my powers of registration at all equal to the powers of reasoning or execution, I should have no difficulty in an inconceivably short space of time in composing a voluminous table of logarithms."[1]

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[1] Proceedings Civ. Eng., XV, 259.

The least intelligible of all the explanations given by ready reckoners is that of Colburn. His friends tried to elicit a disclosure of the methods by which he performed his calculations, but for nearly three years he was unable to satisfy their inquiries. He positively declared that he did not know how the answers came into his mind.[2] In London he made a couple of explanations. "In one case he was asked to tell the square of 4395: he at first hesitated, ... but when he applied himself to it he said it was 19,316,025. On being questioned as to the cause of his hesitation, he replied that he did not like to multiply four figures by four figures; but, said he, I found out another way: I multiplied 293 by 293, and then multiplied this product twice by the number 15 which produced the same result. On another occasion, when asked the product of 21,734 multiplied by 543, he immediately replied, 11,801,562; but, upon some remark being made on the subject, the child said that he had, in his own mind, multiplied 65,202 by 181 [$21734 \times (181 \times 3) = (21734 \times 3) \times 181$]."[3]

Finally, it is worthy of remark that the attempt has been made to teach the performance of long multiplications without writing more than the problem and the answer. Although the method proposed is undoubtedly not the best, yet it suggests the possibility of inventing a practicable school-method. For example, multiply in one line 2681475 by 93165. Number the figures with indices as shown:

$$\begin{array}{r} {\scriptstyle 7\,6\,5\,4\,3\,2\,1} \\ 2681475 \\ {\scriptstyle 4\,3\,2\,1\,0} \\ 93165 \\ \hline {\scriptstyle 11\,10\,9\,8\,7\,6\,5\,4\,3\,2\,1} \\ 249819618375 \end{array}$$

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[1] Proceedings Civ. Eng. XV, 255.

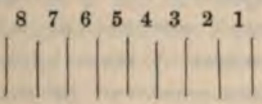
[2] Memoirs, p. 39.

[3] Analectic Mag. I, 1813.

Place the product of any two figures in that place of the result which has an index equal to the sum of the indices, of course adding in any carried numbers. Thus, $\overset{0}{5} \times \overset{1}{5} = 25$; the sum of their indices being 1 the 5 goes in the first place. $(\overset{0}{5} \times \overset{2}{7}) + (\overset{1}{6} \times \overset{1}{5}) + 2 = 67$; the 7 goes in the second place. $(\overset{0}{5} \times \overset{3}{4}) + (\overset{1}{6} \times \overset{2}{7}) + (\overset{2}{1} \times \overset{1}{5}) + 6 = 73$; the 3 goes in the 3rd. place, etc. Such a method of multiplication would undoubtedly be of assistance in training the ability for mental calculation. Of course we do not advocate an attempt at doing such enormous problems wholly in the mind, but shorter ones can be easily learned to great advantage. We should, however, take a few hints from Bidder, Safford, Colburn, *et al.* Suppose we had 379 × 42. Let us mentally index the numbers as above; then 4 × 3 = 12, which belongs in the 4th place. $3 \times 2 = \overset{3}{6}$; add this on to the other, 126. $4 \times 7 = \overset{3}{28}$; add to 126=154. $7 \times 2 = \overset{2}{14}$; add on to 154=1554. $4 \times 9 = \overset{2}{36}$; add, = 1590. 9 × 2=18; add on, =15918, Ans.

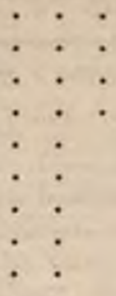
$$\begin{array}{l} \overset{321}{379} \\ \overset{10}{42} \\ \hline \overset{4321}{12} \\ 126 \\ 154 \\ 1554 \\ 1590 \\ 15918 \end{array}$$

Mentally the figures would not be repeated, but as Bidder explains, the first obtained result would be modified. As a help in learning to keep the correct places, a card with several numbered compartments might be placed before the eyes, at first actually, then mentally; thus,



For paper or slate this of course requires more time and figures; but mentally such a process is quite possible, whereas the ordinary way of multiplying 3 figures by 2 figures is absolutely impossible to an ordinary person. With practice boys of advanced classes could undoubtedly be taught to multiply even 3 figures by 3 figures in the head.

R. A. Proctor, using Colburn as an illustration, explains the feats of calculating boys by an increased power of picturing a number as so many things and of modifying this picture according to the operation to be performed.[1] 24 would be presented as



If 24 were to be multiplied by three all that is necessary is to picture three sets of dots; then to conceive the imperfect columns brought together on the right, giving six columns of ten and three columns each of four dots; and these three give at once (by heaping them up properly) another column of ten with two over: in all seven columns of ten and one of two,—that is, seventy-two. Proctor, who remarks that "all good calculators have the power of picturing numbers not as represented by such and such digits, but as composed of so many things,"[2] and who once possessed this power in no inconsiderable degree, says of this example that it "takes long in writing, but as pictured in the mind's eye, the three sets representing 24 formed themselves into the single set representing 72 in the twinkling of an eye."[3]

The suggestion is ingenious but it is only a suggestion. Unfortunately for Proctor's attempt to explain how the ready-reckoners reckoned, several of them have given extended accounts of the processes employed by them. The appendices to Colburn's Memoirs (of which Proctor did not know, for he says, "if Colburn had retained his skill until he had acquired power to explain his method, etc.") give an account of his methods of multiplying, extracting roots, etc., which flatly contradicts Proctor's explanation. In regard to Bidder Proctor afterwards admits that there was no room to doubt that his processes of mental arithmetic were commonly only modifications of the usual processes.[1] Proctor arrived at this opinion on the evidence furnished by persons who had known Bidder (Bidder's own account was unknown to him). It is quite a confirmation of the theory of rapid calculation I have proposed, to find that the explanation of Bidder's powers advanced by Proctor is contained as one of the parts of my explanation, which is founded on the first hand evidence of Colburn, Bidder, Dase, etc.

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[1] Cornhill Mag. XXXII, p. 163; Science Byways, p. 349.
[2] Belgravia, XXXVIII, p. 451.
[3] Cornhill Mag., XXXII, p. 163; Science Byways, p. 350.

Among the other mathematical operations in which the prodigies distinguished themselves more or less is the extraction of roots of numbers. In the first place it is to be remarked that Buxton knew nothing of this operation, and on the one occasion on which such a problem was given him he succeeded only approximately after a long time, apparently by running over the squares of various numbers till he found the one nearest to the given square.[2] Dase liked to extract the 5th root, "because he had noticed that in the fifth power the units are the same as in the root. I saw that with our system of numbers the $(4n + 1)$ power has the same units as the root, a rule of which his is only a single case (for $n=1$)"[3]

In an appendix to his Memoirs, Colburn attempts to explain his methods of finding square and cube roots and of factoring. His rule, first formulated two years after he began, was as follows: Find a number whose square ending with the last two figures of the given square; then, when the given square consists of five places, what number squared will come nearest under the first figure (when 6 places, then the first two figures, when 7, the first 3, etc.) of the given square. For example:

What is $\sqrt{92,416}$?
1. What number squared ends in 16? Ans. 04.
2. What number squared comes next to 9? Ans. 3.
Square root, 304.

What is $\sqrt[3]{321,489}$?
1. What number cubed ends in 89? Ans. 67.
2. What number cubed comes nearest to 32? Ans. 5.
Cube root, 567.

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[1] Belgravia, XXXVIII, p. 456.
[2] Gent. Mag. XXIII, p. 557.
[3] Briefwechsel zw. Gauss und Schumacher, V, 382.

Colburn gives a table of the numbers which squared produce any given termination; to each termination there are four possible roots (to 25 there are 10) from which he must choose; *e. g.* a number ending in 16 can have one of the roots 04, 54, 46, or 96. "It is obvious that it requires a good share of quickness and discernment, in a large sum, to see which of the four roots . . . . is the right one."[1] The table for cube roots is very much simpler. These methods are of use only when the given number is an exact square or cube. Both depend on the last two figures, and a person would "probably greatly confuse the calculator by merely adding a small number to the square or cube."[2] Nothing ever excited so much surprise as the facility with which Bidder extracted square and cube roots. "Yet there is no part of mental calculation for which I am entitled to less credit. In fact, it is a mere slight of art." "Nearly every example proposed to me was a true square or cube; hence I hit upon the following expedient. . . . . "[3] He then gives a method exactly like that of Colburn.

It is not necessary to enter into the question how the prodigies found the factors of numbers. Colburn's process is found *in extenso*, on p. 183 of his Memoirs. It is clumsy and involved; he himself allowed it to be a "drag of a method."[4] Bidder's methods are explained on pages 272, 273 and 274 of Vol. XV of the Proc. Civ. Eng.

There is one other characteristic of the association of numbers that meets us in some of the persons under consideration, namely, the firmness with which long series of arithmetical associations cling together. This is seen in the independence of a process of reckoning among other activities and other processes of reckoning. Of Mondeux we read that his thoughts were as strongly directed to the arithmetical operation he had to perform as if he were completely isolated from his whole environment.[5] Buxton would talk freely whilst doing his questions, it being no molestation or hindrance to him.[1] "He would suffer two people to propose different questions, one immediately after another, and give each their respective answers without the least confusion."[2] In this not so very uncommon ability of doing two things at once the mathematicians seem to be specially favored. Dirichlet, for example, says "that he established the solution of one of the difficult problems of the theory of numbers, with which he had for a long time striven in vain, in the Sixtine Chapel in Rome while listening to the Easter music."[3]

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[1] Memoirs, p. 181.
[2] Hamilton's letter in Graves' Life of Sir Wm. R. Hamilton, p. 78.
[3] Proceedings Civ. Eng. XV, p. 266.
[4] Graves, Life of Sir Wm. R. Hamilton, p. 78.
[5] Comptes rendus, XI, p. 956.

Mathematical Inclination. The peculiar fascination for performing arithmetical calculations is sometimes a source of pleasure in itself; a distinguished savant during a public meeting undertook the multiplication of two long lines of figures and explained his action by "the pleasure it would give him to prove his calculation by division."[4] At the sight of figures, geometrical diagrams, and above all, algebraic formulas, young Galois was seized with a veritable passion for the abstract truths hidden behind these symbols.[5]

Even after Safford had lost his powers he continued to find pleasure in taking large numbers to pieces by dividing them into factors, or in satisfying himself that they were prime.[6] The younger Bidder remarks, "With my father as with myself the mental handling of numbers or playing with figures afforded a positive pleasure and constant occupation of leisure moments. Even up to the last year of his life my father took delight in working out long and difficult arithmetical and geometrical problems."[7]

In regard to special inclination to mathematics and its relation to ability for calculation, and also to other abilities, great diversity is shown by the persons we have considered. They can be variously grouped:

1. Those having strong mathematical inclinations with great powers of mental calculation (not necessarily rapid):

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[1] Gent. Mag. XXI, p. 347.
[2] Gent. Mag. XXI, p. 61.
[3] Kummer, Gedächtnissrede, etc., p. 34.
[4] Èloge d' Ampère, Smithsonian Report, 1872, p. 112.
[5] Magasin pittoresque, 1848, t. XVI, p. 227.
[6] Belgravia, XXXVIII, p. 456.
[7] Spectator, 1878, p. 1634.

here we should include nearly all arithmetical prodigies, although Colburn took no satisfaction in answering questions by the mere operation of mind; unless questioned, his attention was not engrossed by it at all; the study of arithmetic was not particularly interesting to him, but it afforded a very pleasing employment.[1] Nevertheless, the fascination for calculation was in some cases overpowering. Gauss considered mathematics the queen of the sciences and arithmetic the queen of mathematics; Buxton had neither eyes nor ears for anything else, and Mondeux and Dase greatly resembled him.

Corresponding to this class we might point out more than one distinguished mathematician who had not the ability to calculate; indeed, it would not be going too far to say that nine out of ten mathematicians have at least no liking for reckoning.

2. Those with inclination and ability for mathematics, including arithmetic: Nickomachos, Gauss, Ampère, Safford, Bidder.

3. Those with special inclination and ability for arithmetic alone;

a. having had no opportunities for other branches of mathematics: Fuller, Buxton, Mangiamele;

b. in spite of opportunities: Colburn, Dase, Mondeux.

c. where the talent disappears before opportunity for development is possible: Whately.

MATHEMATICAL PRECOCITY. "There are children, I know," says Arago, "whose apathy nothing seems able to arouse, and others again who take an interest in everything, amuse themselves with even mathematical calculations without an object." There are still others more seldom than either of these classes, who confine their interest to mathematical calculations alone. Strange as the fascination for arithmetic seems, it becomes still more so when it is manifested at an age at which it is normally absent; strangest of all is the union of ability to the inclination.

Hamilton included calculation in an all-sided precocity;[2]

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[1] Memoirs, p. 69.
[2] Graves, Life of Sir Wm. R. Hamilton, Dublin.

Pascal's ability was for geometry,[1] as was also Clairaut's.[2] With Whately, Colburn, Bidder, Mondeux and Mangiamele the precocity showed itself alone in calculation; the same is true for Gauss's first years. Ampère and Safford, however, resemble Hamilton in showing inclination and ability for the most varied pursuits; the difference being that the mathematical side showed itself in Hamilton after the philological.

Special precocity in calculation showed itself (as far as our knowledge goes) at the following ages:

Gauss, 3,
Whately, 3,
Ampère, between 3 and 5,
Safford, 6.
Colburn, 6.
Prolongeau, 6½,
Bidder, 10.
Mondeux, 10,
Mangiamele, 10,

It is remarkable that in nearly every case (possibly with the exception of Colburn and Whately) the arithmetical prodigies showed rather an extraordinary ability to learn calculation, not an ability to calculate before learning.

IMAGINATION. One peculiarity in the imaginative powers of the arithmetical prodigies is worthy of remark, namely their visual images. Bidder said, "If I perform a sum mentally it always proceeds in a visible form in my mind; indeed, I can conceive of no other way possible of doing mental arithmetic." This was a special case of his vivid imagination. He had the faculty of carrying about with him a vivid mental picture of the numbers, figures and diagrams with which he was occupied, so that he saw, as it were, on a slate the elements of the problem he was working. He had the capacity for seeing, as if photographed on his retina, the exact figures, whether arithmetical or geometrical, with which he was occupied at the time. This faculty was also inherited, but with a very remarkable difference. The younger Bidder thinks of each number in its own definite place in a number-form,[3] when, however, he is occupied in multiplying together two large numbers, his mind is so engrossed in the operation that the idea of locality in the series for the moment sinks out of prominence.[4] Is a number form injurious to calculating powers? The father seems to have arranged and used his figures as he pleased ; the son seems to be hindered by the tendency of the figures to take special places. It would be interesting to know if the grandchild, who possesses such a vivid imagination and in whom the calculating power is still further reduced, also possesses a number-form. The vivid, involuntary visualizing seems to indicate a lack of control over the imagination, which possibly entends to figures, and this perhaps makes the difference.

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[1] Vie de Pascal par Mme. Perier.

[2] Nouv. Ann. de Math,. Paris, 1861, 1$^{\text{ière}}$ serie, XX, Bulletin, p. 50.

[3] Plate I, Fig. 20, in Galton's Inquiries into Human Faculty.

[4] Galton, Inquiries, etc., p. 134.

Colburn said that when making his calculations he saw them clearly before him.[1] It is said of Buxton that he preserved the several processes of multiplying the multiplicand by each figure of the lower line in their relative order, and place as on paper until the final product was found. From this it is reasonable to suppose that he preserved a mental image of the sum before him.

Of the other calculators we have no reports. Children in general do their mental problems in this way. Taine relates of one, that he saw the numbers he was working with as if they had been written on a slate.

The well-known case of Goethe's phantom, the case of Petrie, who works out sums by aid of an imaginary sliding rule, the chess-players who do not see the board, etc., are instances of the power of producing vivid visual imaginations that can be altered at will.

III.

Can we learn anything of practical use from the prodigies? The following points suggest themselves for consideration :

1. Bidder, Safford and the African brokers all speak for the fact that under cultivation the power of mental calculation could be greatly developed ; the immense saving of time in school and afterwards that would result from an ability to shorten the associations, to use a multiplication table of two figures, and above all to register mentally, is sufficient to justify a trial.

2. Fuller, Ampère, Bidder, Mondeux, Buxton, Gauss, Whately, Colburn and Safford learned *numbers and their values* before figures, just as a child learns words and their meanings long before he can read. Bidder declares emphatically, "The reason for my obtaining the peculiar power of

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[1] Med. and Philos. Journal and Review, New York, 1811, p. 22.

dealing with numbers may be attributed to the fact, that I understood the value of numbers before I knew the symbolical figures. . . . . In consequence of this, the numbers have always had a significance and a meaning to me very different to that which figures convey to children in general."[1]

3. Ampère, Bidder and Mondeux learned their arithmetic from pebbles. Arago says of Ampère, "It may be he had fallen upon the ingenious method of the Hindoos, or perhaps his pebbles were combined like the corn strung upon parallel lines by the Brahmin mathematicians of Pondichéry, Calcutta and Benares, and handled by them with such rapidity, precision and accuracy."

The Roman *abacus*, the Chinese *swanpan* and the success of the numeral-frames used in our primary schools, seem to point to the fact that it is best to teach "calculation" (*i. e.*, "pebbling" from Lat. *calculi*, pebbles), before "ciphering." The Arabic *tsaphara*, cipher, means empty; Arabic numeration, however, was considered mysterious by the people of the middle ages, and remains mysterious to many a child of to-day; to the former (and also not seldom to the latter) "ciphering" meant a secret and unintelligible process. If we could do away with the mystery of calculation perhaps the values of numbers and the tables might become then so indelibly fixed in the minds of children and so easy of application that they also could do long "sums" mentally or even carry the two-figure multiplication tables in their heads.

4. Dase's power of quick apprehension suggests the extension of the training sometimes attempted in schools, in which a slate with letters or figures is shown for an instant to the scholars who are then required to tell how much they recognized.

In conclusion it is necessary to express my obligations to President Hall and to Dr. de Perrot. To Dr. de Perrot of the Mathematical Department of this University, credit must be given for proposing the subject, for a large part of the references, and for numerous valuable suggestions and points of information.

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[1] Proceedings Civ. Eng. XV, 256.

THE PSYCHOLOGY OF TIME.

By Herbert Nichols.

III.—Experiments at Clark University.

In October, 1889, I was requested by the instructor in Psychology at Clark University to investigate the apparently contradictory results obtained by various experimenters regarding the Constant Error of Time-judgments. As a preliminary, the methods of previous experimenters were tested, until after several weeks, a single, and perhaps crucial point seemed to stand out as the proper question upon which to concentrate investigation, namely, the effect upon our estimation of any particular interval of previous sustained exercise or practice upon some other interval. A long series of experiments was then regularly undertaken which lasted several hours daily, for a period of over nine months of actual experimental work. 27 persons were tested; over 500 "sittings," or series of reproductions were made, comprising a total of approximately 50000 single judgments recorded. Five lengths of interval were chiefly used, namely: .25, .50, .75, 1.25, 1.75, seconds.[1]

Apparatus: After trying different metronomes in various ways, these were abandoned as inaccurate. Previous to beginning our regular experiments a nearly perfect instrument for beating time was found in a pendulum constructed as follows: A stiff bar, thin but wide, and five feet long, swung upon knife edges projecting from opposite sides a little above the middle of the length of the bar, and resting upon smooth metal plates, was supported by firm frame-work. Upon each end of the bar was a heavy 'bob' or weight which could be slid up or down and fastened with a spring and clamp-screw at any distance from the point of support. With the first pendulum made, any length of interval could be obtained, by proper adjustment, from half a second to two seconds, beyond which, beats could be regularly omitted from the electric circuit to be described, thus securing intervals of any length desired. The lower end of the pendulum-rod bore a platinum needle that at each swing made electric connection, at the centre of the pendulum arc, with a mercury meniscus. This pendulum, once set in full swing by the hand, would, for medium-length intervals, preserve regular beats for a far longer time than any single set of experiments, without any discoverable variation whatever. Great care was taken at each change of the interval to adjust the 'bobs' and mercury contact so as both to make the interval of exactly the stated length, and the back and forth swings precisely equal, these being the two matters needing the nicest adjustment in all pendulum motion. The pendulum was introduced into the same electric circuit with an ordinary telegraph key, a telegraph sounder, and a Deprez signal which wrote on the drum of a Ludwig kymograph with automatic spiral thread for the revolving drum. Another Deprez signal wrote the vibrations of a tuning fork upon the same drum, by means of a separate circuit and a König contact. For adjusting the intervals and beats for the first time, a fork of 100 double vibrations was used; the adjustment was extended through one hour, until a beat was secured, the sum of whose error was indistinguishable for that space of time, and therefore the error for any set of experiments practically zero. Two other pendulums were also made for shorter intervals, one of them giving quarter seconds. Any two of these pendulums could be introduced into different loops of the same circuit, and each being adjusted to a different interval, either of the intervals could by means of a bridge, be sent through the same sounder at the will of the operator and without stopping either pendulum; or again at will both pendulums could be cut out of the circuit altogether. The reproductions or judgments of the person undergoing experimentation were expressed by a slight movement of the finger upon an electric key that, by another Deprez signal in a separate circuit, recorded the judgment upon the kymograph drum. Thus during each set of experiments three electric signals with points arranged over one another, precisely in the same line at right-angles to the motion of the drum, continuously wrote their separate records as follows: Number one recorded the vibrations of a tuning-fork; number two, the beats of whichever length of interval the subject was hearing from the pendulum sounder; and number three, the judgments of this interval expressed by the subject. The tracings on the drum were "fixed" and preserved.

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[1] As before, the unit throughout this section is one second, except where specifically stated to the contrary.

As above stated the length of the reproduction was measured by tuning-fork vibrations written upon the drum; for all the experiments except those of table E, a fork was used making 50 double vibrations per second, thus recording hundredths of a second; for table E, which concerns intervals longer than the others (1.75), a fork of 25 double vibrations, recording fiftieths of a second was used. Many methods were tried for saving the enormous labor of counting these vibrations, which task, together with its strain upon the eyes for such a long series of experiments as the present, can only be appreciated by one who has tried it for several months. The slightly irregular motions of the kymograph make it entirely inaccurate merely to scale the intervals. The quickest and safest method of counting we discovered was as follows: When the paper is cut from the drum it presents on the sheet several parallel lines. Several scales were made fitting all the degrees of irregularity which the fork vibrations in these lines from time to time displayed; one of these scales was then selected to fit each line, part of a line, or set of lines according to their variation; usually three, and often one scale would fit the fork-record of a whole sheet; the eye quickly detects, after some experience, whether the scale fits or not, and thus enables the counting of the vibrations by using the scale as a tally, with comparative facility and absolute accuracy.

It is an important feature that in all experiments to be reported, great pains was taken to keep the persons experimented upon, in entire ignorance of the character of their judgments, or of any of the 'points' or the nature of the experiments whatever, in order to secure absolute freedom from unconscious prepossessions or subjective influences; where this was not accomplished, as was necessarily the case in two instances, (subjects S. and L.), there was from the character of the men a minimum probability of subjective prepossessions. Moreover as by far the greater majority of the subjects were thus precluded from prepossession until their tests were completed, and as the records of the few who were not so precluded, including those upon myself, entirely accord with those who were, we think the results are reasonably free from this too usually neglected source of vitiation.

Method: The first class of experiments was conducted as follows: The subject was always seated alone in a noiseless room; the electric sounder and the recording key, both on a table before him, were the only apparatus within his sight or hearing; the former brought him through one circuit the beats of the metronome in sharp metallic strokes of uniform strength; with the latter he recorded his judgments upon the kymograph drum in another room. In the latter room with the kymograph was also the pendulum and remaining apparatus, presided over by an assistant. The precise method of these A experiments was invariably as follows: (1) The pendulum was started with full swing, giving beats .75 in length, the electric circuit remaining open. (2) "Ready" signals passed between assistant and subject. (3) Kymograph and tuning-fork were started. (4) The assistant closed the pendulum circuit long enough to send to the subject six beats, or five intervals of .75 each. (5) The assistant opened the pendulum circuit, silencing the sounder. (6) The subject meantime had sought to catch the beat of the sounder from the first beat of the norm and simultaneously to reproduce the beat upon his recording key during the 6 beats of the norm. After the sounder ceased, he continued to reproduce the interval, without breaking the continuity of the series, according to his closest judgment, these reproductions being recorded continuously by the proper circuit upon the drum. (7) The assistant permitted the subject to continue his reproductions until the drum had exhausted the full length of its spiral, when he signalled "stop." The drum was set to exhaust its spiral in two minutes; thus through all classes of experiments to be reported, the reproductions were extended through approximately the same space of time, though of course the number of reproductions varied according to the length of the intervals used and the judgments made. Frequently short portions of the spiral would be used in adjustments of the signals or by accidents, so that the time actually used was shortened more or less. (8) After a few moments of rest a new beat, .9 long, or 20% longer than the norm was sent in to the subject, which with closest possible attention and care he strove to reproduce simultaneously, stroke exactly with stroke, during three minutes. No record was made on the drum of this exercise or practice. (9) A fresh drum having been put in the kymograph by the assistant during the above exercise, immediately upon the expiration of the three minutes, a signal was given to the subject to cease practicing. (10) A new series of 6 beats of the original norm of .75 was then given, and the above numbers (1) to (7) inclusive were repeated precisely as in their first order. In other words a new drum-full of reproductions of the .75 was obtained under precisely the same conditions as the first, with the exception that the first series was "Without practice" or exercise upon any particular interval, while the second set was under the immediate influence of 3 min. practice upon an interval 20 per cent. longer, *i. e.* on .9 (11) After a proper rest, still a third series or drum-full was taken precisely as before, except this time after like practice upon an interval 20 per cent. shorter than the norm, that is on .6

Thus was obtained at each "sitting," though with proper rest between each series, three sets of judgments, as follows: (*a*) without practice; (*b*) after 3 min. exercise upon .9 intervals; (*c*) after 3 min. exercise upon .6 intervals. Table A is arranged to show the comparative results of these three sets.

TABLE A.[1]

Norm, .75 sec. Practice, 3 min. each on .9 sec. and .6 sec. (20 per cent. longer and shorter). Trials, 17. Persons, 6.

(0), (+) and (—) indicate average reproductions made after hearing 6 beats, separated by a normal interval of .75 sec. (0) indicates averages made without practice; (+) after 3 min. practice on .9 sec.; (—) after 3 min. practice on .6. Where the (+) figure is greater than the corresponding (0) figure or the (—) less than the corresponding (0), the figures are printed heavy, to show that these figures follow the rule that practice on a longer interval lengthens the judgment and practice on a shorter interval shortens the judgment as expressed in a following effort to reproduce the standard interval. The letters heading the vertical columns are the initials of persons acting as subjects. The small figures under each initial give the number of experiments from which the averages are made.

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[1] The exigencies of space in the JOURNAL require the withholding of still more detailed tables carefully prepared and in the author's possession.

Set.	S. 5	L. 3	C. 3	F. 3	A. 2	N. 1	General Averages 17
(0)	.712	.607	.750	.735	.671	.814	.712
(+)	.710	**.663**	.727	**.757**	**.749**	.801	**.723**
(—)	.715	.614	**.697**	**.706**	.680	**.731**	**.689**

Results: With normal interval of .75, the general average of 17 tests upon 6 persons shows that there is a very slight and uncertain tendency to follow the rule that three minutes previous close attention to, and simultaneous reproduction of, intervals respectively 20% longer or shorter than the norm, correspondingly lengthen or shorten the judgment; that is, that the habit formed by the practice holds over to influence the succeeding judgments but slightly, if at all.

Series A being deemed inconclusive, it was followed by Series B, the only changes made being first, that a norm of 1.25 was used in place of .75, and second, that only two sets of reproductions were taken, namely: one without practice (0) and one after three minutes practice (—) on an interval of .25.

TABLE B.

Norm, 1.25 sec. Practice, 3 min. on .25 sec. Trials, 60. Persons, 12. At the head of each vertical double column is the initial of the subject. In the left hand column are the numbers of the single experiments from which the averages in the other columns are computed. The columns headed (0) contain average judgments of the 1.25 norm made without practice; those headed (—) similar judgments made after three minutes practice on a .25 beat. This table shows the average for each set of each individual, and also the general averages of each individual and of the total experiments of this table. The averages for this table are computed from the full number of reproductions of each drumful.

No. of Trial.	N.		S.		C.		L.		F.		A.	
	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)
1	1.423	1.328	1.332	1.275	1.244	1.146	1.245	1.166	1.597	1.517	1.138	1.212
2	1.453	1.303	1.470	1.403	1.229	1.169	1.238	1.091	1.437	1.425	1.379	1.319
3	1.530	1.317	1.748	1.343	1.278	1.276	1.222	1.258	1.491	1.446	1.368	1.349
4	1.321	1.252	1.313	1.306	1.316	1.307						
5	1.376	1.248	1.493	1.176	1.281	1.253						
6	1.333	1.287	1.519	1.328	1.335	1.280						
7	1.278	1.252	1.625	1.550	1.323	1.267						
8	1.249	1.189	1.515	1.522	1.334	1.307						
9	1.234	1.216	1.282	1.156	1.437	1.312						
10	1.346	1.277	1.226	1.209								
11	1.285	1.229										
12	1.336	1.185										
13	1.245	1.209										
14	1.350	1.196										
15	1.396	1.164										
16	1.337	1.265										
17	1.278	1.193										
18	1.296	1.186										
19	1.362	1.257										
20	1.381	1.287										
Gen'l Average.	1.335	1.242	1.435	1.313	1.306	1.253	1.236	1.167	1.506	1.461	1.291	1.290
Difference.	—.092		—.121		—.053		—.068		—.044		—.001	

TABLE B.—Continued.

No. of Trial.	W.		M.		Sh.		K.		Ca.		D.	
	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)
1	1.137	1.201	1.169	1.189	1.352	1.275	1.311	1.280	1.138	1.118	1.441	1.288
2	1.355	1.284	1.239	1.206	1.147	1.216	1.256	1.245	1.146	.954		
3	1.246	1.199										
Gen'l Average.	1.246	1.226	1.203	1.192	1.242	1.241	1.284	1.261	1.142	1.022	1.441	1.288
Difference.	—.020		—.011		—.001		—.023		—.120		—.534	

Results: Total General Average Without Practice, 1.3228"
" " " After " 1.2533"

Difference, .0695"

These B experiments upon the 1.25 interval, show an almost universal shortening of those judgments which were preceded by three minutes close attention to, and simultaneous reproduction of, beats .25 long, the average difference between the judgments of the two conditions being .0695. The average difference of no individual out of the 12 included in the table varied from the general rule, and only in 6 trials out of the 60 was the rule broken for single trials, and no person broke the rule more than once. In general, those most experienced in laboratory work conformed most strictly to the usual law; the law was most frequently broken upon the first test made upon an individual, this happening 4 times out of the 6; and it may be remarked in relation herewith, that more variations should be looked for from nervousness or other disturbing causes under these conditions, and from those persons with whom they were actually found. In general, also, the amounts of the difference made between (0) and (—) was proportional to the amount of experience the subject had in psychophysical experiment; for instance, those for Dr. Donaldson, Dr. Sanford, Dr. Lombard and myself are among the largest. Curves were drawn for each individual similar to those of the accompanying chart. Study of these discovers that the Constant Error, whether plus or minus, shows itself most frequently to a marked degree, from the very beginning of the reproductions, and nearly always so before the seventh to the ninth beat, or in other words, before the elapse of ten seconds. Also, the Constant Error tends to preserve a uniform course from the beginning, either the judgments growing gradually longer or gradually shorter throughout the drum, according as their value is greater or less than the normal; in those individuals where the Constant Error is greatest and most marked, this gradual increase or decrease is most marked, as with Dr. Donaldson, where is the largest plus value, and with Dr. Lombard, where is next to the greatest minus value of the judgments.

A beat .25 in length was now chosen for the norm, and being shorter and more difficult to catch was always given 10 times as a sample for each set of reproductions, in place of 6 beats, as in the previous experiments. The practice interval was also changed for this table to 1.25, and for a period of 5 minutes in place of 3 minutes, as formerly. The reason for this increase in the length of the time of practice is manifest when we consider that two factors enter into the functions of practice, namely : first, the number of repetitions which the subject or central cells would be called upon to make during the practice ; and secondly the fatigue, nutritive, restorative, or other processes, which may depend somewhat upon the mere length of time which the practice is continued. We know little or nothing of the effects of either factor, but as in the C experiments practice on 1.25″ gave much fewer number of repetitions, the length of practice time was increased from 3 to 5 minutes, which was an indefinite compromise between proportional length of time of practice, and proportional number of beats.

The shortness of the interval would have given a great number of reproductions, since the same length of the drum's spiral was used as before ; and the labor of counting so many would have been excessive ; therefore, although the subject made his reproductions for approximately the same length of time as in the previous experiments, records were taken upon the kymograph of only the first 40 reproductions, and of a second set of 40, taken after the lapse of one minute from the last beat of the norm. All the other conditions were the same as before, making the method for Table C as follows : (1) Norm of .25 (10 beats given); a drumful of reproductions taken without practice. (2) Practice 5 minutes on 1.25 beats. (3) Norm of .25, (10 beats given); a drumful of reproductions taken after practice.

TABLE C.

Norm, .25 sec. Practice, 5 min. on 1.25 sec. Trials, 30, Persons, 8. Shows averages of each set and trial, of each individual, and the general averages as before. Averages of the first 40 reproductions are marked *a*, of the second 40, *b* ; and the average of *a* and *b* is marked *c* ; (0) without practice ; (+) after 5 min. practice on 1.25.

No. of Trial.		S.		N.		H.		Ma.	
		(0)	(+)	(0)	(+)	(0)	(+)	(0)	(+)
1	a	.259	.289	.238	.241	.273	.289	.249	.261
	b	.245	.288	.230	.243	.232	.297	.242	.249
	c	.252	.288	.234	.242	.252	.293	.245	.255
2	a	.254	.255	.247	.248	.244	.262	.246	.249
	b	.239	.250	.243	.247	.225	.262	.238	.242
	c	.246	.253	.245	.247	.235	.262	.242	.245
3	a	.249	.251	.256	.248	.242	.259		
	b	.244	.244	.245	.242	.227	.251		
	c	.246	.247	.251	.245	.234	.255		
4	a	.261	.260	.237	.239				
	b	.243	.255	.234	.240				
	c	.252	.257	.235	.240				
5	a	.263	.265	.242	.252				
	b	.258	.255	.234	.248				
	c	.260	.260	.238	.250				
6	a	.259	.263	.246	.254				
	b	.254	.253	.238	.250				
	c	.256	.258	.242	.252				
7	a	.249	.265	.254	.261				
	b	.236	.262	.254	.259				
	c	.242	.264	.254	.260				
8	a	.253	.247	.242	.248				
	b	.256	.248	.217	.234				
	c	.255	.244	.230	.241				
9	a	.249	.246	.237	.246				
	b	.234	.245	.208	.237				
	c	.242	.246	.222	.241				
10	a	.245	.251	.243	.254				
	b	.234	.254	.231	.251				
	c	.239	.252	.227	.252				
Totals.	a	.254	.259	.244	.249	.253	.270	.248	.255
	b	.254	.255	.233	.245	.228	.270	.240	.246
	c	.249	.257	.239	.247	2.40	.270	.244	.250
Differences.	a		.005		.004		.017		.006
	b		.011		.011		.042		.006
	c		+.008		+.008		+.029		+.006

TABLE C.—Continued.

No. of Trials.		Ca.		B.		McD.		T.	
		(0)	(+)	(0)	(+)	(0)	(+)	(0)	+
1	a	.244	.252	.255	.253	.248	.249	.258	.252
	b	.233	.253	.246	.250	.251	.252	.256	.245
	c	.238	.253	.250	.252	.250	.250	.257	.249
2	a	.246	.251						
	b	.250	.254						
	c	.248	.252						
Totals.	a	.245	.251	.255	.253	.248	.249	.258	.252
	b	.241	.253	.246	.250	.251	.252	.256	.245
	c	.243	.252	.250	.252	.250	.250	.257	.249
Differences.	a		.006		.001		.000		.006
	b		.012		.004		.000		.011
	c		+.009		+.002		+.000		—.008

Results: Total General Average Without Practice (*a*) .2500; (*b*) .2396; (*c*) .2448.

Total General Average After Practice (*a*) .2557; (*b*) .2525; (*c*) .2542.

Difference (*a*) .0057; (*b*) .0129; (*c*) .0093.

These C Experiments seem to show that 5 minutes' practice upon a 1.25 beat, lengthens judgments of .25 intervals on an average .00935; the result is the more striking and conclusive when the smallness of the average lengthening is compared with its constancy, the "after practice" set of General Averages of the total 30 trials, exceeding the "without practice" set in every instance, and even in averages of three trials, as those of H (a subject who at the time was entirely ignorant of the purpose of his experiments), the "after practice" judgments falling below the corresponding "withouts" but twice out of the 240 recorded judgments. The Curves of the General Averages of the total thirty trials is shown in Fig. III of the Chart, and those of H in Fig. IV. The continuous line in the chart shows the judgments "without practice," and the dotted line "after practice" as previously in Figs. I and II.

TABLE D.

Norm, .75 sec. Practice, 7 min. on 1.75 sec. and 5 min. on .25. Trials, 30. Persons, 8.

This table will be understood without other explanation than that its method was precisely that of Table A, except that the 'long' practice was changed from 3 min. upon .9 to 7 min. upon 1.75, and the 'short' practice from 3 min. upon .6 to 5 min. upon .25; also, 7 beats of the norm were given for the copy from which the reproduction of each set was made. The table shows averages of each set and trial, of each individual, and the General Averages as before. Averages of the first 40 reproductions are marked *a*, of the second 40 *b*, and the average of *a* plus *b* is marked *c*; (0) = without practice; (+) after 7 min. practice on 1.75; (—) = after 5 min. practice on .25.

No. of Sittings.		S.			N.			H.			B.		
		(0)	(+)	(—)	(0)	(+)	(—)	(0)	(+	(—)	(0)	(+)	(—)
1	*a*	.725	.786	.710	.801	.803	.747	.798	.764	.666	.705	.685	.648
	b	.726	.788	.690	.800	.814	.759	.835	.869	.670	.629	.621	.599
	c	.725	.787	.700	.800	.808	.753	.817	.817	.668	.667	.653	.623
2	*a*	.748	.835	.772	.781	.771	.751	.800	.878	.744			
	b	.766	.882	.792	.773	.798	.761	.899	.963	.794			
	c	.757	.859	.782	.777	.785	.756	.849	.920	.769			
3	*a*	.793	.795	.699	.739	.846	.712	.810	.982	.774			
	b	.801	.803	.718	.753	.844	.722	.894	1.148	.900			
	c	.797	.799	.709	.746	.845	.717	.852	1.065	.837			
4	*a*	.718	.795	.721	.736	.836	.696	.732	.909	.732			
	b	.699	.809	.754	.731	.975	.676	.794	1.056	.779			
	c	.708	.802	.737	.734	.906	.686	.763	.985	.755			
5	*a*	.768	.924	.727	.822	.826	.740	.787	1.003	.736			
	b	.746	1.019	.729	.790	.889	.716	.833	1.233	.794			
	c	.757	.971	.728	.806	.857	.728	.810	1.118	.765			
6	*a*	.782	.877	.735	.777	.804	.709						
	b	.761	1.120	.705	.775	.810	.704						
	c	.772	.999	.720	.776	.807	.707						
7	*a*	.781	.889	.778	.771	.820	.708						
	b	.824	1.118	.819	.770	.990	.693						
	c	.803	1.003	.798	.770	.905	.701						
8	*a*	.714	.816	.716	.761	.972	.722						
	b	.704	.822	.696	.798	.997	.727						
	c	.709	.819	.706	.780	.984	.725						
9	*a*	.758	.879	.744	.753	.961	.741						
	b	.768	.852	.743	.758	.952	.735						
	c	.764	.865	.743	.756	.956	.738						
10	*a*	.754	.840	.726	.747	.819	.788						
	b	.780	.855	.716	.751	.867	.777						
	c	.767	.847	.721	.749	.843	.782						
Totals.	*a*	.754	.843	.732	.768	.845	.731	.785	.907	.730	.705	.685	.648
	b	.757	.907	.736	.769	.893	.727	.851	1.053	.787	.629	.621	.599
	c	.755	.875	.734	.767	.869	.729	.818	.980	.758	.667	.653	.623
Differences.	*a*		+.089	—.021		+.076	—.037		+.121	—.055		—.020	—.057
	b		+.149	—.019		+.123	—.042		+.202	—.064		—.008	—.030
	c		+.119	—.021		+.100	—.040		+.162	—.059		—.014	—.044

almost universal shortening of those judgments which were preceded by three minutes close attention to, and simultaneous reproduction of, beats .25 long, the average difference between the judgments of the two conditions being .0695. The average difference of no individual out of the 12 included in the table varied from the general rule, and only in 6 trials out of the 60 was the rule broken for single trials, and no person broke the rule more than once. In general, those most experienced in laboratory work conformed most strictly to the usual law; the law was most frequently broken upon the first test made upon an individual, this happening 4 times out of the 6; and it may be remarked in relation herewith, that more variations should be looked for from nervousness or other disturbing causes under these conditions, and from those persons with whom they were actually found. In general, also, the amounts of the difference made between (0) and (—) was proportional to the amount of experience the subject had in psychophysical experiment; for instance, those for Dr. Donaldson, Dr. Sanford, Dr. Lombard and myself are among the largest. Curves were drawn for each individual similar to those of the accompanying chart. Study of these discovers that the Constant Error, whether plus or minus, shows itself most frequently to a marked degree, from the very beginning of the reproductions, and nearly always so before the seventh to the ninth beat, or in other words, before the elapse of ten seconds. Also, the Constant Error tends to preserve a uniform course from the beginning, either the judgments growing gradually longer or gradually shorter throughout the drum, according as their value is greater or less than the normal; in those individuals where the Constant Error is greatest and most marked, this gradual increase or decrease is most marked, as with Dr. Donaldson, where is the largest plus value, and with Dr. Lombard, where is next to the greatest minus value of the judgments.

A beat .25 in length was now chosen for the norm, and being shorter and more difficult to catch was always given 10 times as a sample for each set of reproductions, in place of 6 beats, as in the previous experiments. The practice interval was also changed for this table to 1.25, and for a period was strong and constant. Figure V of the chart shows the curve of the General Averages for the 30 trials and 8 persons; figure VI shows the curve for Sh., and illustrates a single trial.

TABLE E.

Norm, .5 sec. Practice, 5 min. on 1.75 sec. Trials, 6. Persons, 2. The only other variation than those in the above line was for these experiments, that 10 beats of the norm were given for the sample from which each set of reproductions was made. Averages of the first 40 reproductions are marked *a*, of the second 40, *b*; the averages of *a* plus *b* are marked *c*; (0) = Without Practice; (+) = after 5 min. practice on 1.75.

No. of Trials.		S.		N.	
		(0)	(+)	(0)	(+)
1	*a*	.517	.541	.491	.521
	b	.503	.518	.484	.515
	c	.510	.530	.487	.519
2	*a*	.491	.528	.503	.523
	b	.505	.559	.483	.516
	c	.498	.544	.493	.520
3	*a*	.497	.535	.489	.498
	b	.504	.551	.484	.477
	c	.500	.543	.487	.487
Totals.	*a*	.501	.534	.494	.514
	b	.503	.542	.466	.502
	c	.502	.538	.480	.508
Differences.		+.033 +.039 +.036		+.020 +.035 +.028	

Results: Total General Averages Without Practice (*a*) .4980; (*b*) .4852; (*c*) .4916.

Total General Averages after practice on longer beat (*a*) .5246; (*b*) .5228; (*c*) .5237.

Total General Average difference (*a*) .0266; (*b*) .0376; (*c*) .0321.

Figure VII of the chart shows the curve for the general averages of the six tests of these experiments on the interval .5.

TABLE F.

Norm, 1.75. Practice 6 min. on .5. Trials, 6. Persons, 2.
Seven beats of norm given for sample to be reproduced. (0) = Without Practice; (—) = after 6 min. practice on, .5 sec.

No. of Trials.	S.		N.	
	(0)	(—)	(0)	(—)
1	2.03	2.14	2.19	1.83
2	2.06	2.36	2.24	1.79
3	2.31	2.35	1.88	1.75
Totals.	2·136	2.284	2.049	1.790
Difference.	+.048		—.259	

Results: Total General Average Without Practice, 2.089
" " " After " 2.010

" " " Difference, — .079

It will be observed that the three trials of S. for this interval are all contrary to the usual law; whether this is accidental and due to the small number of the trials, or if practice is less efficient in its influence upon judgments of long intervals, is undetermined; we incline to believe the former.

At this point in the experiments it appeared conclusive that a certain amount of sustained exercise, with close attention to the repetition of definite beats heard from a pendulum or sounder, and reproduced by motion of the finger upon a key, induces some sort of more or less permanent effect or habit, whose influence unconsciously modifies accordingly the judgments or reproductions of other beats heard and reproduced immediately after such exercise or practice. The question now arose whether this effect was muscular or "central." To determine this, the following experiments were instituted; their method was the same as the foregoing except that in place of hearing the beats of the sounder the armature or stroke-bar of the latter was pressed lightly between the thumb and forefinger of the *left* hand; the soft parts of the balls of the fingers were intruded slightly between the bar and the anvil or brasses between which the bar played, and, the circuit being closed, each time the pendulum made a stroke a "pulse-like" sensation was felt by the fingers. The left hand, thus holding the sounder, was then rolled in several thicknesses of cloth and folded with a woolen coat, and the ears closed with cotton or wax till no noise from the sounder could be heard with the closest possible attention. Also, the practice was now exercised or received in a purely afferent manner, without repeating the practice interval upon the key, simultaneously with the beats of the sounder as was done in the other experiments. By these means the effect of the practice was confined afferently to the left thumb and forefinger, and to their respective nervous centres. The reproductions of the trial intervals, both the set previous to practice and the correlative set after practice, were made with the right hand or fingers, as in all previous experiments.

It is evident that if similar effects from practice should manifest themselves under these conditions as in the former experiments, the cause could in no way be attributed to a muscular habit, because no muscles were at all concerned in the reproductions of the normal or trial intervals, which had been in any way influenced by the previous afferent exercise on the practice interval. Of course it is possible that every afferent impulse occasions some efferent discharge, although the same be actively ineffectual; yet even if this did happen, we think it would be fair to assume that the cause of the difference between the two sets of judgments was central and not muscular.

TABLE G.

Norm, 1.25. Practice, 6 min. on .25. Trials, 50. Persons, 16.

Practice taken by touch alone in left thumb and finger, the beat being inaudible. Ten beats of norm given as sample for all reproductions. (A) = Averages without practice; (B) = Averages after 6 min. practice on .25 beats; D = difference between (A) and (B).

TABLE G.

No. of Sittings.		S.	N.	L.	H.	C.	A.	D.	B.
1	*A*	1.276	1.200	1.065	1.480	1.339	1.146	1.433	1.442
	B	1.215	1.109	.942	1.530	1.390	1.111	1.437	1.334
	D	− .061	− .091	− .123	+ .050	+ .051	− .035	+ .004	− .108
2	*A*	1.302	1.174	1.025	1.443	1.564	1.396		
	B	1.202	1.033	1.238	1.410	1.475	1.393		
	D	− .100	− .141	.203	.033	− .089	− .003		
3	*A*	1.247	1.140	1.358	1.447	1.289	1.446		
	B	1.195	1.029	1.159	1.282	1.438	1.480		
	D	− .052	− .111	− .199	− .165	+ .149	+ .034		
4	*A*	1.207	1.082	.883	1.388	1.307	1.493		
	B	1.350	1.058	.882	1.223	1.255	1.420		
	D	+ .043	− .024	− .001	− .165	− .052	− .073		
5	*A*	1.390	1.245	1.093	1.333	1.277	1.553		
	B	1.281	1.071	1.012	1.233	1.284	1.369		
	D	− .109	− .174	− .081	− .100	+ .007	− .184		
6	*A*	1.174	1.179						
	B	1.109	1.085						
	D	− .065	.094						
7	*A*	1.350	1.326						
	B	1.353	1.202						
	D	+ .003	− .124						
8	*A*	1.322	1.261						
	B	1.289	1.089						
	D	− .033	− .172						
9	*A*	1.583	1.271						
	B	1.335	1.154						
	D	− .248	− .117						
10	*A*	1.334	1.213						
	B	1.361	1.031						
	D	+ .027	− .182						
Totals.	*A*	1.310	1.210	1.061	1.416	1.347	1.389	1.433	1.442
	B	1.262	1.083	1.029	1.326	1.363	1.312	1.437	1.334
	D	− .048	− .126	− .032	− .090	+ .016	.077	+ .004	− .108

TABLE G.—Continued.

No. of Sittings.		McM.	W.	Ha.	M.	Mi.	Ma.	Hn.	Ca.	Average of last 10.
2	*A*	1.301	1.350	1.408	1.372	1.425	1.029	1.183	1.268	1.306
	B	1.163	1.210	1.397	1.474	1.322	.906	1.194	1.161	1.245
	D	— .138	— .140	— .011	+ .102	— .103	— .123	+ .011	— .107	— .061
Totals.	*A*	1.301	1.350	1.408	1.372	1.425	1.029	1.183	1.268	1.306
	B	1.163	1.210	1.397	1.474	1.322	.906	1.194	1.161	1.245
	D	.138	— .140	— ·011	+ .102	— .103	— .123	.011	— .107	— .061

General Average, { 1.277
1.213
— .064

Results: Total general average without practice, 1.2776
Total general average after 6 min. practice on, .25, received only through left thumb and finger, } 1.2137

Total general average difference, —.0639

The results of these *G* experiments are particularly to be compared with those of Table *B*, both having had the same norm and same practice intervals. The length of time which practice was undergone, however, was in *G* double that in *B*, which probably should be counted as a reason why the difference between the "without practice" and the "with practice" results should have been greater in *G* than in *B*. An offset to this influence, however, lies probably in the fact that in the *B* practice the intervals were not only afferently received, but also efferently executed, bringing into play the whole psychophysical arc of sensory centers, motor centers, and muscles of the fore-arm, hand and fingers; under these circumstances this arc soon takes on, as a whole, a simultaneous function of a strongly reflex nature, the reproductions not following the beats of the norm, but precisely and spontaneously coinciding with them, beat on beat; the whole process of reproducing here is itself of the nature of an induced habit, and it is natural to suspect that the continuation of the habit, sustained through the term of practice, would have a stronger and more lasting effect than where the sensory centers alone were exercised, as in the *G* experiments. What the precise results of these countervailing conditions may have been we cannot determine, but the very close equivalence of the total differences of the two tables (— .0695 for *B*, — .0639 for *G*), is very likely to have been within certain limits accidental.

It is not likely that the same experiments repeated under conditions as nearly as possible like these, and upon the same individuals, would produce precisely the same results, for the human organism, mental and physical, is so complex, its environment so variable, the entire conditions of the problem so multifariously changeable, that the mathematical probabilities are almost infinitely against identical combinations. But results constantly like in nature and approximately like in degree, should, we think, be deemed scientifically acceptable. Even with these, the time-problem is so difficult and so liable to subjective and delusive complications, that we cannot look upon the experiments here reported, (as extensive, careful and conclusive as we have endeavored to make them,) as being entirely conclusive until they shall have been confirmed by similar work of other experimenters. With these provisions, however, we think the results of the foregoing experiments indicate that, *sustained attention to a rythmically repeated impulse induces in the corresponding nervous centre a habit or tendency to continue that impulse, which habit influences, or modifies succeeding time-judgments.*

The following table summarizes our results with reference to the Constant Error. We have thought best to give the length of the judgments rather than the amount of the error; the plus sign is prefixed to those judgments which are greater, and the minus signs to those which are less than their corresponding norm; also, the table shows the number of trials from which each average is calculated, and the table from which the same are taken. The judgments of Table *II* are alone the first series of each set or trial, that is, those made without practice or normally.

TABLE H.—CONSTANT ERROR.

Persons.	Norm .25		Norm .50		Norm .75		Norm 1.25		Norm 1.75	
	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.
S.	10 C	— .249	3 E	+ .502	5 A 10 D Average	— .712 + .755 — .741	10 B 10 G Average	+1.435 +1.310 +1.373	3 F	+2.136
N.	10 C	— .239	3 E	— .480	1 A 10 D Average	+ .814 + .769 + .773	20 B 10 G Average	+1.335 —1.210 +1.293	3 F	+2.049
L.					3 A	— .607	3 B 5 G Average	—1.236 —1.061 —1.126		
C.					3 A	+ .750	9 B 5 G Average	+1.306 +1.347 +1.321		
H.	3 C	— .240			5 D	+ .818	5 G	+1.416		
A.					2 A	— .671	3 B 5 G Average	+1.291 +1.389 +1.353		
Ca.	2 C	— .243			1 D	— .673	2 B 1 G Average	—1.142 +1.268 —1.184		
Ma.	2 C	— .244			1 D	— .659	1 G	—1.029		
Sh.					1 D	— .740	2 B	—1.242		
Mi.							2 B 1 G Average	—1.203 +1.425 +1.277		
F.					3 A	— .735	3 B	+1.506		
W.							3 B	—1.246		
D.							1 B 1 G Average	+1.441 +1.433 +1.437		
B.	1 C	+ .250					1 G	+1.442		
K.							2 B	+1.284		
McD.	1 C	+ .250								
T.	1 C	+ .257								
Bl.					1 D	— .667				
McM.							1 G	+1.301		
W.							1 G	+1.350		
Ha.							1 G	+1.408		
M.							1 G	+1.372		
McA.					1 D	— .725				
Hn.							1 G	—1.183		
General Averages.	30 C	— .244	6 E	— .491	17 A 30 D Average	— .712 + .760 — .742	60 B 50 G Average	+1.322 +1.277 +1.302	6 F	+2.089

Results: With the method used, the experiments, on the whole, indicate that the judgments of intervals of .75, .50 and .25 are very slightly shortened, while those of 1.25 and 1.75 are considerably lengthened. Too few intervals were used to determine the Indifference Point accurately, yet in view of the great variations displayed, we may perhaps come as near the truth as can be well attained, if we calculate this point, for these experiments, from the General Average of the intervals used; according to such computation the Indifference Point would appear to be about .81. Yet so great are the individual differences and even the variations of the Constant Error from time to time for the same individual, that this error should be termed Inconstant rather than Constant, and as calculated from any number of persons yet experimented upon, must be considered as extremely problematical and uncertain. Particularly so, as we entirely lack any sure clue to its probable cause. In view of the indication arrived at, that the phenomenon is central, we might infer that the lengthening of the judgment was due to an inertia or tardiness of the centres to repeat the proper rhythm, and that this might be based upon a failure of response in nutritive processes; but this would be difficult to reconcile with the fact that the more rapid intervals, which would be supposed to exhaust the centres most quickly, display the opposite tendency and act more quickly than they ought. Or perhaps the relations between the nutritive and active functions of the centres, are an automatically compensating mechanism, wherein the supply is sometimes "over corrected" and again "under corrected" with reference to the exhaust, just as the balance wheel of a watch is often at fault with reference to temperature, and the watch varies with the season and with the pocket it is carried in; so the time-mechanism of the nervous centre may vary with individual and physical conditions, and with the coat we wear; surely the psychical timepiece is not less delicate or complex than its horological rival of human skill.

Comparing our own results with those of former experimenters, though we learn next to nothing of the cause of the Constant Error and too little of its course to predict

dict the same with any great probability, for any certain person or number of persons; yet study of our tables, and still better of the original curves and charts too numerous to publish, reveals a few points of considerable certainty. Those individuals who make the largest constant error, make the error most constantly in one direction; such persons, also, are apt to make a constantly increasing error throughout the series of reproductions of each drumful; this phenomenon betrays itself even more conspicuously in the "after practice" series than in the "without practice" series; the phenomenon is illustrated in the judgments of L and of H in Table *G*, and in their respective curves, Figures X and XI of the chart; judgments of the former are unusually short throughout the experiments, and in the curves, show themselves growing rapidly shorter and shorter to the end of the drum; the judgments of H are unusually long throughout all his trials, and his curves go rapidly up throughout each drum. This raises a serious question as to what the magnitude of the Constant Error would be for a longer and different period of reproduction. Possibly, also, this point has relation to the fact that contrary signs are found for the Constant Error by the German experimenters who used single reproductions, and by Mr. Stevens (with whom my results pretty closely agree) and myself, who used multiple reproductions. Examination of the first reproduction of each drumful of my work, does not discover the contrariness of sign for Constant Error, between the first and the subsequent judgments of the series, which would correspond to the contrariness of results between the above mentioned experiments with single and with multiple reproductions. New experiments seem needed for the tripartite relations between the sign of Constant Error, the number or length of time the norm is given as a sample, and the number of the reproduced judgments.

Another feature of interest is, that any slight nervousness or excitement of the subject shortens the judgments. Often the subject who sits for the first time, looks upon any psychological experiment as in some way a test of mental caliber; this, together with fresh interest in the experiment, occasions a slight eagerness, excitement, or mental tension for the first trial, which is not so much, if at all, present in future ones. Examination of results taken under such conditions, convinced me while the experiments were in progress that they were shorter than the ordinary ones. It is evident that this, if true, would have bearing upon the method of our experiments; for instance, if in first sittings the average judgments of the first or "without practice" trial be for the above reason shortened more than the following "after practice" set, allowance ought to be made for this in estimating the shortening or lengthening effect of the practice upon the later set; otherwise, in those cases where the practice interval was shorter than the norm, the shortening effect of the practice in the "after practice" set would be negatived to the extent of the shortening due to excitement in the "without practice" set, and the reverse for practice intervals longer than the norm. Examination of the tables shows that the law, that the "after practice" sets are longer or shorter than the "without practice" sets, according as the practice interval is longer or shorter than the norm, is broken to a more or less degree in 48 out of 246 times; 17 out of these 48 digressions occurred in first sittings, and 11 out of these 17 occurred in those experiments where the practice interval was shorter than the norm. This is in accordance with what has been said regarding excitement, yet a more detailed scrutiny of the results than is possible to give here, is chiefly the ground for what we have stated on this point.

Much has been said by previous experimenters concerning the effects of attention. Undoubtedly with single reproductions sensibility and accuracy are directly proportional to the attention given; with multiple reproductions it is doubtful if this is the case for the expert and experienced subject. For myself, who have had very unusual experience, my best judgments are made by paying the greatest possible attention to the norm during the sample beats, and then, when the rhythm is once caught, abandoning myself to as near an unconscious or reflex condition as possible, letting the idea or habit of the rhythm run its own course undisturbed, as near as may be, by attention, volition, or any kind of thinking whatever.

Subjective opinions of one's own judgments: After finishing each drumful the subject throughout the experiments was usually asked his opinion of how well he had kept his copy or norm; only in a small and uncertain number of cases were these opinions found to agree with the truth, and frequently were directly contrary.

How long before the effect of practice shows itself as against the immediate memory of the norm? The results are so variable that this question cannot be answered with precision; nearly always the effect of the practice is exhibited in the very first reproduction to a marked degree; before the expiration of 8 or 10 seconds the effect would seem to be in full force or tendency, from which time forth, the judgments where the Constant Error was well marked, gradually grew longer or shorter to the end of the drum, as we have before stated.

How long does the effect of practice last? Our method did not permit us to observe a longer period than from 1.5 to 2 minutes; the practice seemed to preserve its effect with nearly, if not entirely, its full force for that length of time.

Fatigue: A few experiments were made preserving closest possible attention to the beats and judgments for several hours at a sitting; sample tests of the judgments were taken from time to time. So far as these go, fatigue could not be discovered to have any effect whatever.

Long Experience in making time judgments has been thought by Mehner and others to lessen the Constant Error. Study of the above experiments according to their dates on the protocol, which also agree with the order of the tables as published, discovers very uncertain evidence for this opinion, a slight probability perhaps inclining in its favor.

Mr. Stevens noticed in his work, that judgments of unusual length or shortness are apt to be corrected in the following judgment, "according to a standard which the mind carries, but to which the hand (or perhaps the will during the interval) cannot be accurately true." To a certain degree the same phenomenon is observed in my charts and curves, though I am rather inclined to carry back the cause to some automatically compensating adjustment of the rhythmic habit or function of the nerve centres, than to the vague phrase "a standard carried by the mind."

Anomalies: Seeing no just reason for the culling out of anomalies in former experiments, I have permitted none in my own. Every test taken in the course of any regular experiment has been reported in its proper place, with the exception of a single trial each, for three persons, who, from nervousness (one was a young woman) or lack of rythmic sense, were entirely unable to catch the beat of the norm in a way that would enable them to repeat it with any sort of regularity or likeness to the original.

Sensibility: Owing to the enormous labor that would be involved in computing the Average Error for so many judgments, no investigation was made by me of this factor. On the whole, however, I should say the nearly uniform results regarding sensibility of all former experimenters, which constitute almost their sole point of agreement, are entirely confirmed by the experiments here reported.

In closing this account of my experiments I have pleasure in thanking those who have given me so much valuable time, taken from their own University work, in acting as subjects for such a tedious and time-robbing investigation, and those also who have assisted me by suggestion, counsel and inspiration.

IV.—CONCLUSIONS.

Sensations and their images or reproductions have various attributes; qualitatively they are blue, or warm, or painful etc.; intensively they are strong or weak, bright or faint, etc. Duration, or continuation, is another attribute or characteristic of every sensation and of every image. This attribute is the ultimate and essential datum of time. Besides sensations and images, science infers and assumes the real and separate existence of certain physical elements, having fixed correlations with each other, and with sensations and images. Whether the grounds for this assumption are acceptable or not we need not here discuss; but according to this assumption, duration or continuing is also an attribute or characteristic of these physical elements, and therein forms a further field of this ultimate and essential time datum. Again most philosophies, and, I think, all religions and all science, assert, infer, or assume the existence of some soul or super-psychical cause, as an ultimate element separate from, or as a further attribute additional to, the physical elements and the sensations and images; according to these grounds there is thus another field of this characteristic time datum. Thus our time datum is seen to be an attribute belonging to, and inherent in, everything that is conceived to exist. As such, also it is seen to be an ultimate datum; as much so as the blueness, the chilliness, or the painfulness of any sensation, or the existence of anything at all. Why things exist at all, or why their inherent nature is what it is, we think to be at present beyond human explanation. The fundamental datum of our present explanations, then, we shall state to be that time *is* this attribute of duration wherever it exists.

This being the nature of time, what constitutes a perception of time? Hoping the results will justify the use, we shall accept that nomenclature according to which it is said that every elementary sensation or image is *perceived* which presents itself in consciousness at all. When a sensation or image properly occupies the focus of attention, we shall say it is *apperceived*. According to this terminology, time is perceived whenever any sensation or image durates[1] in consciousness at all; it is apperceived when the duration properly occupies the focus of attention. Thus if we suppose a creature so simple as to be without memory, and capable from time to time of but a single elementary sensation of constant quality, say a pain, (such perhaps are some infusoria) we should say that pain was *perceived* whenever it occurred; we should not say it was *apperceived*. We should also say such a creature *perceived* time.

Why sensations ultimately differ at all, why some are red and some blue, some bright and some faint, or why some are long in duration and some short, is beyond explanation. That some are long and some short is an ultimate datum, and no more wonderful than that sensations are diverse in any other way. But in the same way as we shall say of our simple creature, that he perceives his sensation when it exists at all, and that he perceives time when it (the sensation) durates at all, so we shall say he perceives a certain definite time when it durates in that certain definite manner. Its perception is its occurrence; the ultimate nature of its occurrence, constitutes the ultimate nature of the perception; the definiteness of its occurrence, of its inherent nature, constitutes the definiteness of that certain perception. We know nothing of the perception of such a creature except by inference and analogy; but in the same way that we should say his sensation is painful, in that same way we should say one of his perceptions was five seconds long. And in the same way that we have said he perceives time when his sensation durates at all, so we shall say he perceives five seconds when it durates five seconds, and perceives one second when it durates one second. But according to this, one thing above all else must be carefully noted, *perception* or perception of time duration is *always a process and never a state; a certain definite time is a certain definite process.* We can no more discover an explanation of our perception of the duration of five seconds alone in some mysterious momentary mental arrangement or "temporal sign," or other *instantaneous* characteristic, than we can discover redness in blueness; for us to perceive blue, there must be blue; for us to perceive duration, something must durate; for us to perceive a definite blue, there must be a definite blue; for us to perceive five seconds, something must durate five seconds; for us really to *perceive* a year, some definite sensation would have to durate a year. What takes place when we say we have an *idea* of a year is another matter which we shall discuss in its place.

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[1] I have coined this word, finding no other sufficiently simple in meaning.

So also of series of sensations. That series occur at all is an ultimate fact or datum. What actually occurs when a series occurs we shall call a perception of a series. And in the same way as we can never perceive a half-second except something durate a half-second, so we can never perceive a series of five half seconds with intervals of half seconds between the terms, unless such a series occurs. When it occurs its entire occurrence will constitute its perception. Actually to perceive such a series a year long, such a series would actually have to occur throughout a full year. What takes place when we have an idea of such a series we shall also discuss in its turn.

Neglecting for the present any consideration of the correlation between them, or of any perception of such a correlation, all that we have said regarding sensations applies as well to images or reproduced sensations; really to imagine five seconds, some image must last five seconds; fully to imagine a thousand clock-ticks, a thousand clock-tick images must pass through the mind. So also, fully to remember a thousand second-beats, a thousand second-beat images must pass in full mental review.

And as of pains, and clock-ticks, and second-beats, so of all other mental content whatsoever and however disparate. Mental process is mental perception; every definite or certain process or procession is a definite and certain perception; and every definite perception is also a definite time perception. Yet we must not forget that according to the nomenclature we are now using, perception is not apperception, and a definite time perception is by no means an apperception of a definite time; this we shall come to later.

What we have said of perception applies as well to memory. But when we *say* we remember an occurrence, we seldom, and indeed never, except the occurrence is short, simple and of recent happening, remember it as accurately and fully as it actually transpired. That is, in its re-presentation in memory, some of the items drop out of the process, or rather fail to drop into it; and the remainder stand *unsuspected* for the former whole — do so for the very reason that the former whole now is not, nor can be suspected at all, except in and through so much as *is* re-presented. I may have spent the whole of yesterday listening to the second beats of a clock, yet I may quickly remember that I did so, without the entire day and each tick repeating itself in full or in any instantaneous miniature of fullness in that quick remembrance. But in this quick remembrance, it is probable the entire mental procession of the previous day was re-presented alone by some momentary flash-picture, as it were, of myself as I was seated at some particularly striking moment of yesterday, listening to the clock; perhaps this flash-picture or remembrance lasted long enough to take in no more than two represented ticks of the clock; perhaps to take in but one; or it may be that *all* the image-ticks were left out entirely and only the word "tick" or "clock" occupied their place in the quick remembrance; for such, it seems quite certain, is the nature of much of our thinking. If that in the above quick remembrance which occupies the place of, stands for, indicates, or symbolizes the original series be named the *idea* of that series, then the idea of that series is not a full representation of that series in any way. And it is plain also that we have in such an idea no such occurrence as that described by Herbart, or Mr. Ward, or any of those who conceive that an idea of a series, or of succession, or of time, must be some sort of instantaneously painted picture presenting the whole length of the time or of the series in a simultaneous perspective. Indeed if needed at all, there would seem to be needed as much such an instantaneous sidewise view of the duration of the simplest sensation and of the briefest part of time in order to perceive that it durate at all, as to perceive that it durates for, say, five seconds. The classic question therefore whether the idea of succession is or is not a succession of ideas, in so far as the question is one as to whether the idea is a longitudinally passing *process*, or a sidewise presented *state*, may as well be fought out with reference to the nature of any original sensation and for the briefest temporal portion of it, as with reference to any train or series of such sensations. Whether a sensation, an image, or a series of such, it does not matter; the pertinent question is, do we perceive the length of any duration, however long, by the *process* of that duration itself, or by some non-processional representative state? The chief arguments or suggestions I have been able to formulate or to find formulated for the "state" theory, all root, it seems to me, in the delusive catch-phrase, "We can not *now* perceive something that is *really past*, therefore our perception of past must be a *present* perception, *i. e.* a state." But this phrase is a series of verbal mis-statements and bad logic from beginning to end; we do not "*now*" perceive this something, whatever it is, but so far as I can discover we "now-now-now-now" perceive it; we do not stand still and look along the line to measure this past in a perspective view, but *run* along the line as it were (a new line representing the old) to measure it inch by inch, or present by present, by a moving process over again; nor is this something that we re-measure a "really past," nor in the absolute sense do we *re*-measure at all; but the "really past" and the original measuring both gone forever, a new representation of the gone past and a new measuring of the new representation happen "brand-new;" happen in original *representation* of them, though not in re-*presentation* of them. All this being so, our phrase carried out in good logic should read "We *can not* 'now' perceive something that is really past, therefore our 'perception of past' must *not* be a present perception, *i. e. must* be a *process*. On the other hand, the evidence for the opposite or "process" explanation seems to me consistent and even positive. I think that every one who will observe his own mental process when he seeks to measure or to realize the length of any durating sensation or its representation in memory, will easily observe that he never fully perceives or remembers the length instantly or even approximately so; unless, of course, the duration is itself instantaneous or approximately so. On the other hand I think any one will easily convince himself that *fully* to perceive or to remember the length of its representations, these representations must stretch themselves out through an equal process and lapse of time as did their original occurrence. 'Quick ideas' of the nature described above may delusively flash upon us with approximate instantaneousness, but never a full and complete idea, and the time occupied by the idea will be proportional to its completeness.

Another evidence in favor of the process and against the "state" explanation lies, we think, in the following facts. The items of a long series, say the detailed events of a past hour, never are fully represented to us. It is easy to account for this according to the process theory; many of these details fail to reappear, and as the serial reappearance of those which do reappear is our sole suspicion of their presence, or of their order of appearance past or present, so of those which fail to appear, we *at the time* have no suspicion of their absence or of the fact that they ever existed. At some other time we may remember further details, and also remember this abbreviated memory, and so become aware that we have dropped items from the latter. But according to the "state" theory, it is difficult to conceive why those causes which give the proper perspective to any part of a series where no items are gone, should not give the proper *perspective* to those items which do appear in a series when some items do not appear, and why such a perspective state would not have much such an aspect as the perspective of a picket-fence where some of the pickets are on and some off. Nor must we imagine such a conscious running-over of yesterday's incidents, as one in which we skip or jump from one incident to the other and almost feel the shocks occasioned by gaping items, to be just such a broken-fence perspective as we above describe. Surely such a series of shocks are a process and not a simultaneous state, even if we are conscious of the gaps; but how we come to *be* conscious of the gaps in this running perspective, is a complex question entirely separate from the one under present consideration, and one we shall hope to throw light upon later. That we do not have simultaneous picket-fence perspective with pickets visibly off, that is with perspective gaps belonging to lost items, is quite evident in our attempts to recall the precise number of ticks in a given series just heard; where as those who have had much experience must observe, they frequently with confidence think they recall the whole series perfectly and with no consciousness of gaps, though there are gaps.

We are inclined to conclude therefore that by the same process that we perceive the duration of any smallest part of any single sensation, by precisely the same nature of process we perceive the duration of any sensation however long and of any series however long; that the duration of the sensation or series, the perception of the duration, and the perception of the length of the duration are one and identical; that the duration is an ultimate datum, and no more capable or needful of other explanation or of further analysis than the blueness of a blue spot.

But perception of the length of a sensation, the apperception of its length, and the perception and apperception of its length as measured by some other sensation, are different matters altogether, as also are so-called perceptions of past, present, and future, and of other definite time relations, and of dates; all of which we must now consider.

More often than otherwise those definite sensations which come through the focus of the eye are those which determine the immediately following ideas; with great frequency these sensations definitely persist long enough to associate for some time with the ideas which they call up. With less frequency the definite sensations of hearing, touch, smell, and so on down the scale, determine the immediately following associations. Frequently very obscure sensations such as a red spot at the very edge of the field of vision, or the temperature of our teeth direct the association. Or perhaps as often as otherwise the particular mental group which determines the association is not a sensation or procession of sensations, but a definite group of images or procession of images which we may call an idea. Whatever group it is that determines the succeeding association, that group we say occupies the focus of attention, the terminology being evidently derived from the fact that the focus of vision is so frequently also the focus of apperception. Apperception is complete association; the object of association is always the object of apperception, and the object of attention. The focus and the object are always identical. When we apperceive anything we couple it with its most usual associations, that is, memories of its own attributes, qualities, and characteristics. This kind of association *is* apperception. Time is apperceived when any process of duration occupies the focus of attention, is the object of association, and calls up durative associations; that is, memories whose characteristics are particularly of the duration quality or nature.

We must with GREATEST *care distinguish between perceiving time and apperceiving time relation.*

Perceptions of relation are commonly supposed to be involved in the very core of the indissoluble mystery of the unity of the mind. We are deeply aware of the importance of the subject, yet we have been driven to suspect that the secret of perceived relations is to be found in that they are associative processes of the apperceptive degree or nature and not simultaneous states. This subject is not our main question and we shall discuss it but in so far as is necessary for our explanation of time relations. If two tones precisely alike in quality, intensity, and length, begin precisely together and end together, no relation will be perceived between them. If one begins perceptibly before the other, relations will appear. Without some qualitative or some intensive *change* there can be no temporal *relation*. The occurrence of the change in the qualitative or intensive nature of the perception *is* the perception of the relation; and in the same way as it is not some necessary sidewise simultaneous perspective that constitutes perception of homogeneous duration, but the ever flowing attribute of duration itself, therefore we suspect, that every perception of temporal relation is fundamentally the actual procession of one term of definite quality or intensity followed without gap by another term of different quality or intensity; that actually to perceive any definite time relation or change, such must actually transpire; andfully to imagine or to remember such, the corresponding representation of it must again pass through the mind in full review. Without qualitative or intensive change no series could occur; such change is the essential characteristic of a series; the change *makes* the series. Fully to perceive the relations of the terms of a series the full series must be experienced either in original occurrence or in representation. To perceive that A B C D occurs in the relations a b c d it must occur in these relations. To perceive that B is after A, A must happen, then B. To perceive that A is before D, A must happen before D. To apperceive these relations is something quite different. To perceive that D is present and that A B C are gone, A B C must come and go and D must come; the apperceiving of the presence of D, or of the goneness of A B C, or of the relation of the presence of D to the goneness of A are other matters that need much elucidation.

To apperceive D it must occur, stand in the focus of the mind and call up images representing its qualities and usual associations; to apperceive it as *present*, it must call up the idea 'present'; the apperceived relation of D to the Present is the occurrence of D followed by some idea of "the Present." For us to understand this relation we must understand "the idea of the Present." The word "present" is one that we associate with the continued presence of any mental content, or more strictly speaking with the durative procession of that content through the mind; thus we can associate the word with a passing image as well as with a passing sensation; but most commonly the word Present associates itself with the bodily group of *sensations* which we call self and with the environmental sensations which happen to be present at the moment that we are so apperceiving 'the Present.' Thus when we apperceive D as *present* the process that nearly always occurs is something as follows: first D itself, then the word "present," then some durating procession, most probably some sensation procession of our body or our surroundings at the particular moment. The length of this last associated durative procession is variable; in quick apperceiving, as in quick remembering it may be little more than the word 'present' alone; or it may be the quick flash of some mental duration even without the word "present."

But while on this subject we must not let words confuse several distinct data. Strictly the *perceived Present* is the content of any perception at the time of its occurrence; is that occurrence itself. Similarly the *apperceived Present* is the occurring object of apperception; that which directs the association. But to apperceive *the* Present that is, to apperceive the mental content actually occurring *as* the Present, that is again to perceive its *relation* to the Present, this occurring content must call up, and be associated with, the idea "Present"; that is very likely with the word "present" followed by some durating procession very probable to be a continuation of the surrounding stream which was the object of apperception from the beginning; in other words the apperception of the Present *as* the Present is usually but a sustained association of the word "present" with the progressive flow of the sensations within us or from around us. The *change* which reveals the relation, that is, the change which constitutes the relation, in so far as a relation is a psychological occurrence, is the change occasioned by the dawning appearance of the associated idea.

So much for the Present, for apperceiving the Present as the Present, and for apperceiving D as present; now for the 'goneness' of A, B or C—the Past. Strictly speaking, mental content has no Past and no Future; they are only while they are, and their existence is like a mathematically moving point, or speaking of the total content, is like a plane moving at right angles to itself. What then is this moving procession by which, as we say, we have knowledge of the Past?

From what we have discovered regarding Present, we may suspect that perception of Past, perception of past relation or relationship, apperception of Past, and apperception of past relationship are all different matters. As the existence of any temporal portion of any mental content constitutes a perception of Present, so the *cessation* of its existence, constitutes a perception of Past. In order to perceive Past, some sensation or image must *cease*; whenever any such ceases, we perceive Past; the ceasing of the perception is the perception of Past; did no perception ever cease we should never perceive or know anything whatever regarding Past, or pastness, or about *the* Past.

To have a perception of past relation, a *relation*, that is, as we have explained, a *change*, must occur. To perceive a temporal *relation* between A and B, B must be different from A, and to perceive the relationship the relationship must occur; and we shall perceive whatever occurs, and we shall perceive it as it occurs, while it occurs, and in its occurrence; and we shall *only* perceive what occurs and while it occurs, and in its occurrence. To perceive A before B, A must occur and B succeed. To perceive B after A, A must occur and B succeed. The *perceptions* of the relation, A before B and B after A, are identical because the relationship A before B is the relationship B after A. The *apperceptions* described by these two phrases we shall discover may be quite different.

For an apperception of Past, the cessation of some sensation or image must call up some *idea* of Past, of something ceasing; striking or familiar examples, those which most forcibly impress memory, are those most *likely* to be called up; yet the least possible flitting perception of something ceasing would suffice for the associated idea; or even merely some word, such as "past," "gone," etc.

When we come to apperception of past relationships, we arrive upon confusing and difficult ground; not because the essential and typical process is different from all other apperception, but because the associated ideas are so varied in number and kind, and our uses of language so loose and delusive. First we must note that an apperception of Past is not an apperception of past relationship. For example, A may occupy the focus of attention and its cessation call up associations of ideas of pastness. In this case B did not occur at all, and in the associations brought up, the pictures are of single terms ceasing. This is apperception of Past. But to apperceive any temporal *relation* A B, the change A B must occupy the focus of attention and its occurrence call up by association some *idea of relation*; that is, some mental picture of change, some *a* followed by some *b*.

Our space will not allow us to analyse all the apperceptions of temporal relationships of past, nor is it necessary to do so; a few important types will give the key to all. Perhaps the most crucial in the whole time problem is that which takes place, when, as we say, we perceive that something is *of* the Past; a moment *ago* I knocked on my table so hard that it hurt; I heard the knock and then felt the pain. What in my mental process constitutes this "ago"? Clearly the "ago" is a relationship with some present. But what sort of relationship? a perceived or an apperceived one? And with *what* present? Is it with "now-now-now-now"? Or are we to speak of some particular "now," that "*now*" is not "now" at all, but as we shall see a mere idea of "now." When I felt the pain, I was not *thinking* about its time relations, that is I was not apperceiving such. I did *perceive* the time relation of the sound to the pain; I did not *apperceive* it. It is quite possible that a representation of that sound may pop into my head again immediately after actually hearing a similar sound; I shall then perceive a time relation between that representation and that sound, but I shall not necessarily *ap-*

perceive any time relation between them; *whether I do or not will depend on whether the occurrence determines the subsequent current of association; and the kind of relation that may be apperceived will depend upon the kind of associations that are called up.* Suppose I do hear a similar sound at some future time of my life, and by some favorable condition determined by my surroundings or thoughts at that time, a representation of the former sound and of all its surroundings at its time of occurrence be by association again brought into the focus of apperception; that having thus sprung into mind *by* association they should then dominate and determine by association the next and following course of apperception and so on. What takes place here is a present sound like a former sound, followed by a representation that is like it and also like a former sound, followed in turn by a panoramic representation of that certain stretch of my past life that happened when I struck the table, heard the original sound, felt the pain and so on. Thus far there is in all this no apperceived "ago," no apperceived time relation, merely this panoramic representation of the Past is passing through my mind; I have not yet apperceived or, as we say in ordinary language, I have not yet *recognized* that it *is* the Past; no least thought of the temporal *relation* of all this panorama nor of any part of it, not even of the represented knock, to the Present may have yet occurred to me. What shall we say so long as this panorama goes on and no direct time-relation to the Present is thought of? Shall we say all this is nothing but imagination? This question I think brings us to one of the most usual sources of confusion for our entire subject. Usually we *do* call just such panoramas as this memories, and remembrance, whether we do actually stamp their date upon them and think their "ago," or their "how long ago" or not. The vast majority of the representations of those things which have happened but a moment or a few moments before, we have no need to date and do not date. The stream of thought or apperception into which they rise is not one regarding time relations or time characteristics, or time recognition. For instance, had I been writing an explanation of pain instead of time, the same panorama of the table, my hand moving toward it, the thump, the pain, would have occurred as now when I write of time; but from this point instead of a train of associations of time nature being set going, a train of pain relations would have been set going; that is I should have apperceived pain and not time. The mere passage of past panoramas through the mind in no way constitutes a *recognition* that they are of the Past, or of how long they are past. Presently I shall show the difference between imagination and this sort of undated, unrecognized memory; we are now examining dated memories, and we wish to know in what this dating consists over and above the mere passing picture. From what we have discovered, this should be comparatively well understood. First regarding the knock we may merely *think of it past*, without bringing in the Present or any particular time relation; that is we may merely *apperceive it past*: in this case its image or representation will merely bring up in apperceptive process, ideas of Past; the image of the thump will cease and ceasing images will follow; perhaps the representation of the knock will continue to occupy the centre of the stage for some time, will continually go through the process of ceasing and of setting associated images to ceasing; and for the time the whole play will be a regular variety performance of ceasing, while we may or may not be saying all the time or repeating all the while to ourselves the *words* 'past, past, past'; or 'thump past,' 'thump past'; or if we had been less engrossed with the particular performers, any portion of it might have sufficed; a single "tumble" or cessation of the first comedian Thump himself, followed by a tumble or two of the associated company; or if even less engrossed, a mere glimpse of Thump followed by the word "past" would have completed the theatrical bill. This is the simplest form of apperception of the relation Past; the *change* is that from the ceasing thump representation, to the associated ceasing representations; the *pastness* lies in the relational change to the associated idea of ceasing, and this idea is composed of the associated ceasing representations.

Next we may apperceive that the thump happened *before* the pain; here 'Thump then Pain,' 'Thump then Pain' will be the chief theatrical performance, in imitation of the original actual occurrence; if we are not in a critical mood this pantomime may suffice even without the word "before"; the *repetition* of the main performance may constitute the idea; or if we are more reflective and exacting, the whole company may be called out, and the whole stage be set whirling with mimic and peek-a-boo representations of 'beforeness.'

The "show," for apperception of the fact that the pain happened *after* the thump, would differ little from the last above. Pain here would come on to the stage first, making the bow, as it were, that introduces the "show," instead of Thump, as previously, and he will probably make an extra bow between each alternating bout between him and Thump, just to make sure that we keep our eye mainly on him; and every time he makes such a bow he (or we) will say "after," or to speak more soberly, the word "after" will say itself by association, instead of the word "before" saying itself.

The play by which this Thump-Pain representation is apperceived, *i. e.* thought of in relation to the moving Present, may now be easily understood; we here no longer look alone at the stage, but we take in the whole view around us, from our body outward and, as well, from our body inward. Thump-Pain are the chief actors on *the inner stage* as before; they are the *first* objects of apperception from which the course of thoughts wanders momentarily down among the audience, that is to our actual 'now-now-now' surroundings, and inward to our own bodily sensations and even to attention to our own thoughts; but now and anon our focus of attention flits back from these actual Presents to the show 'Thump-Pain,' again viewed on the mimic stage of memory. And as we have said of simple apperception of Past, so of this process of apperception of 'having happened before the Present.' Here the play may be longer or shorter according as we are more or less reflective; a twinge of neuralgia may suffice for the moving Present, with or even without the word "present" or "now"; and a single bow from Thump or Pain, that is a single memory image of these may suffice for their remembrance, or there may follow a full apperception of 'Thump-Pain past,' that is of the *ceasing* of the thump and of the pain, as described above.

We have now described what takes place when, as we say, we think of a thing or event as past, and when, as we say, we think of something as *of* the Past; that is, past with reference to the moving present. But particular time-relations, such as yesterday, last week, a year ago, ten minutes since, remain to be discussed. But as this brings up the subject of *measured* time, let us postpone these for a word concerning so-called perceptions of Future. As the fundamental sign of every idea of Present is the *continuation*, and that of every idea of Past is the *cessation* of some representing image, so the fundamental sign or characteristic of every idea of the Future is the *beginning* of the representing associated images. When I think of the Future of things, I think of them as *beginning*. As I go over a familiar way, memories of the path ahead of me beyond my view keep *rising* in my mind and constitute the foundation of expectation. If I apperceive these expectations, as expectations, the associations are those of the *act* of expectation, plus the panoramas of the path. In this case, *I* enter into the "show," the whole moving action of my bodily feelings while I sit here or walk there and expect; that is, certain holdings of the head, wrinkling of the brows, laying my finger to my chin, or the like; meanwhile the stage show goes on, the performances now being emphatically those of the "beginning" nature or plot, together with little mimic side pantomimes of myself in the acts and experiences of expecting; also the orchestra plays "future" "future" the while, or anon, plays "expectation" "expectation," and the panorama of the path ahead of me moves on in ever beginning glimpses. Apperception of Future, and apperception of *the* future, are similar to the apperception of expectation, and, I think, need no further explanation than may be derived from the above.

But how do we *measure* time length, and measure "how *long* ago," and "how long until?" When speaking of our simple creature capable of but a single constant sensation, we said that when his pain lasted five seconds, he perceived the length of five seconds, and when it lasted one second, he perceived the length of one second. We distinctly declared he did not apperceive either length, and from what we have said of change and relations it is clear that I have not conceived that this creature perceived relations of any kind; neither relation of difference nor of number. Here we must be most careful not to let our customary use of language and our common processes of thought designated by common language, confuse us as to the actual elementary processes of mind which we never experience singly, and for which consequently we have no common or definite designations; and, what is more usual, have no definite or apperceived conceptions. Until some one opened our understanding to the matter, we went on deludedly imagining that we saw distance through rod-and-cone processes, the same as we did blueness; now we discover that what we call *seeing* distance is chiefly not seeing at all. It is probably the same with all the ultimate elements of sensation; Prof. Wundt reminds us that we never experience them singly, and so with great difficulty arrive at any conception of what each or any one element singly of itself is like, or what its various attributes are like. We should be prepared therefore to comprehend, since apperception of length and of number is not perception of length or of number, and again since perception of *difference* of length, and difference of number, (these all involving changes and relations) are not mere plain perception of length and perception of number, we should, we repeat, yet be prepared to comprehend that perception of five-seconds length is not in ultimate nature the same as perception of one second length. That there is a difference here, we think it comparatively easy to demonstrate, though it is quite certain that we do not ordinarily apperceive the difference, that is, do not form and associate ideas of it with its occurrence. It is probable, in our ordinary apperceptions of time length, that the associated *ideas of length*, which make up the apperception, are those representations or memories of muscular tensions, dermal stretchings or joint pullings, which fundamentally are the components of our ideas of motion; consequently has perception of time been so commonly founded on perception of motion, from Aristotle down to present psychology. There is little doubt that the intensive changes, which are the characacteristics of these motion sensations, are the striking and characteristic components of those associated ideas which enter into our ordinary apperceptions of time length. But we must not fail to note that these changes are not the only components of these ideas, and that these image processions, and also their prototype original processions, are not *all* change; there must be duration without change in order for duration with change to be possible. And in the same way that we continually *perceive* changes different in degree of change, without *apperceiving* any difference, so it is probable, and I think certain, that we continually perceive durations of different lengths without apperceiving their difference. For example, of our simple creature I think one should now have no difficulty in conceiving how there might and would be a difference between his perception of a five-second pain and his subsequent perception of a one-second pain, and yet this creature never perceive the difference; that is, might not have any relational idea of such a change, as we might find to constitute the process of perceiving or apperceiving *difference*.

Prepared, therefore, not to confound actual difference with perception of difference, let us examine these matters more closely. We found that duration and change are ultimate data; we shall also discover that differences of duration are also ultimate facts. We shall never discover why ultimately these differences are differences, but given these differences, we shall discover, I think, how we come to perceive, and finally to apperceive, these differences, and in what these processes consist. Carefully considering the matter in the light of the experiments reported in Chapter III, I have been led to suspect that this perception and apperception of durative differences may rise in two ways, which, for convenience, I shall here designate as the single method, and as the multiple method. These experiments emphasized the fact long before determined, that our so-called memory images are dependent upon certain reproductive *habit* processes of our nervous and bodily organism. Were it not for these "habits" we should have no memory. My experiments emphasized *the degree to which the validity of correlation between these so-called memories and their originals, depends upon the validity of these organic habit processes.* If the habit is not accurate, the memory will not be faithful, *although we shall not have the least suspicion that it is not faithful.* The truth is, the memory may be altogether different in temporal length from the original temporal length without our perceiving or recognizing their difference, or suspecting anything about such a difference whatever. Nothing can bring out more clearly than this, that *actual* difference does not constitute recognition of difference, and that perception and apperception and recognition of difference are all some sort of processes quite different from and additional to mere actual difference of occurrence. *To apperceive these differences, they must, by association, bring up certain qualitative ideas and ideas of difference.*

We do not yet know positively the particular portion of the brain organism, whose rhythmic reiterative habits are chiefly responsible for memory; it is sufficient, however, for our present purposes that it is *some* particular portion of nerve organization, which, for convenience, I shall here designate in accordance with present probabilities, as the central nerve cells. My experiments demonstrate that when these cells functionate with reiterative temporal accuracy, our time judgments are accurate, and as their habit varies or is disturbed, our judgments vary correspondingly. We have also to observe how frequency and lateness of original occurrence form and influence this iterative habit. We have then to note, that immediately after the occurrence of a definite sensation, which previously has been frequently repeated, say the tick of a metronome, two forces, or to speak more accurately, the tendencies of two processes, are contending against each other in the production of the succeeding memories; and, indeed, as well in the production of the succeeding *sensations* themselves. The cells, both those which functionate the memories and those which functionate the tick sensation, (be they the same or not, we do not know) tend on the one hand to follow the rhythm to which they have previously been trained, tuned or accustomed, and on the other hand, to adopt a new rhythm in correspondence to the rythmic impulse then and there received from the metronome. Not only, therefore, is the result likely to be ever a compromise between the two, and our sensations at different times and under various conditions, likely to vary from the actual metronome rhythm and from each other, but quite possibly another result of more peculiar nature may also happen from and during this contention of tendencies. For instance, suppose the metronome to be beating quarter seconds and the cells to have been tuned or adjusted by preceding practice according to the method of our experiments to second beats. Plainly by the law of association and habit, the first stroke of the metronome sets going the tendency of the cells to perform their second-beat representations; and consequently the impulses sent in from the succeeding second, third and fourth beats of the metronome will find the cells in a different condition than did the first beat. Precisely what would be the nature of the result of this contention or disturbance of the regular order of things, or what the difference between this and the case where the old habits of the cells should be entirely overcome by the new influence, or where the cells from the beginning were accurately adjusted to the beat coming from the metronome, is difficult to say. It is well to note, however, that this condition of contention between new and old influences or habits is the usual condition rather than the exception; and that any peculiarity of sensation or feeling which should result, as is very likely to result from such a struggle, might be a very important factor in time measurement. Not that such a peculiarity or temporal sign would of itself alone constitute *apperception* of time length, but reproduced representations, or repetitions of these different temporal signs among the associations constituting the apperceptive after-train of ideas called up by *actual* time differences, may be definite and determining data in such apperceptions of different time lengths. And in consequence of these contentions also, and of apperceptions which they determine, it is quite possible that in the *original occurrence* of familiar sensations we may have indefinite cognizance of "too short" or "too long" *without definite memory or apperception of that in relation to which it is short or long;* it is quite possible that these definite memories sometimes are and sometimes are not then called up by these apperceived signs. In short, during the original occurrence of a series we may, as it were, apperceive a general abstract definiteness of length or of time difference or relation, without its being followed by concrete definiteness; that is, we may apperceive that it is definite without apperceiving its full definiteness, for such subtle tricks are, by no means, psychically uncommon. Should that which we have tried to describe be true, those theories which have sought to explain time relations and time perceptions by "temporal signs" or a disparate sense would have herein some foundation of analogy.

But more frequently perhaps are the rudiments of time measurements to be discovered in a method different from the above. Should an image occur simultaneously with its corresponding sensation, the two beginning and ending precisely together, this equality of their length, would, in accordance with our foregoing nomenclature, constitute the perception of their equal length. Without fuller description, we may understand how by association this perception would rise to apperception, and thence to apperception of their temporal equality. Similarly, if the image and sensation were of unequal length, we may comprehend how this would rise to apperception of their inequality. Again, if equal temporal series of simultaneous sensations and memories, or yet again, unequal temporal series of such, occur, we may also prefigure how these get apperceived, and what will constitute the nature of such apperceptive processes. But before we speak finally of such processes, a word must be said as to apperception of number, in order fully to elucidate how we apperceive a sensation to denote *so many units*, or to be so many times longer than another.

For four sensations to be perceived, four sensations must occur; for these to be apperceived, the idea of four, *i. e.*, the word "four," or some four image reproductions, or both the word and the four reproductions must be added in proper apperceptive process thereto. So of any other number of sensations or images. This is the key to the simple apperception of number. A sensation, four seconds long, may occur succeeded by four different sensations, each one second long; by our first method of measuring time length, combined with the apperceptive process of number, we may understand how we arrive at an apperception of one sensation being four times the length of another. Or a sensation four seconds long may occur simultaneously with four sensations, each one second long; and so by the second method of time measurement, combined with the apperceptive process of number, these would rise to apperception of the one as four times the length of the other. And so on with other multiple number-measurings.

Before leaving finally this subject of habit rhythm and time measurement, a word more regarding those theories which have found in our main unconscious bodily rhythms, such as breathing, pulse-beat, and leg-swing, standard rhythmic measures of our time judgments. We have pointed out as objections to these theories, that we have no reason to conceive why one such unconscious process should dominate as a standard more than any other; yet for all to contribute such unconscious disturbances would, indeed, so we must think, lead rather to indiscriminate confusion, than to standard discrimination; such views, moreover, run quite contrary to the selective advantages of unconscious reflex actions, which, by relieving consciousness of all such disturbing vital processes, have made our conscious processes distinct and intelligible. Also we have mentioned that, according to the theories of breathing standards and the like, it would seem that we ought to have a more lively and accurate conception of the definite length of such processes as breathing than of any other duration lengths or rhythms, while, as a notorious fact, we do not; but rather those rhythms which we most customarily hear are those which most vividly rise up with accuracy and as standards. This brings us to the point on which we wish to lay further emphasis; and for this we would note that the particular function, to which our conscious centres seem to be differentiated in contradistinction from the reflex unconscious centres upon which our vital processes depend, lies in just their power and tendency *to adapt* themselves to the multitudinously time varied outer impulses to which consciousness is to correspond, and whose purpose it is to represent; their very peculiarity consists in differentiation to outward susceptibility rather than like the unconscious reflex vital nerve centres to a particular inward rhythm approximately undisturbed by outer influences. Nor must the fact shown by our experiments, that unusual frequency of repetition by the *conscious* cells of impulses received from without tends to perpetuate such particular time rhythms or habits to the temporary detriment of accurate judgments of other rhythms or time lengths received from without, be counted against this view, but rather for it; for if there were *no* tendency for these conscious cells accurately top reserve their habit of repeating the occurrences from the outside, which were their original prototypes, there would never be any accurate time memories or images of our sensations at all, in fact, no rational memory whatever. The whole cerebral and central nervous organism seems a happy adjustment of fixity of habit, not too fixed, and susceptibility, not too susceptible. There would seem reason from *à priori* grounds to suspect, therefore, that which from observation seems to be the case, that our standards of time-measurements are memories of certain most striking rhythmical, habit-inducing, and oft-occurring outer occurrences, such as the particular length of watch or clock ticks, which we are most accustomed to hear; the sounding-hours; the varying lights and shadows of morning, noon and night; the peculiar Sundayness of Sunday and Mondayness of Monday; the varying seasons; perhaps also as we have surmised vague temporal signs or admonitions of passing moments and as well of passing years.

After all the foregoing, it seems unnecessary particularly to explain apperception of such time relations as "yesterday," "to-morrow," "last week," "a week hence," "a year ago," or "ten minutes ago;" these terms are but particular words associated with particular time occurrences and number measurements, which rise into more or less extended and definite processes of apperception of such relations, according to our reflectiveness of mood or passing mental circumstances.

We have seen that much of our thinking is comprised of image-trains representing past occurrences to which we attach no date; which we do not think of or apperceive as of the Past at all; that is, which we do not actually *recognize* as of the Past or as ever having been seen before. We have to repeat that in our belief some of the chief confusions of psychology, and as well of philosophy, come from commonly mistaking this mere passage before us of trains that *are* correspondent to former trains for those mental processes which do properly constitute psychological recognition. It is curious to note that those metaphysicians and psychologists, who most stickle against the possibility of any *real* recognition of any non-psychical *real* world, most unsuspiciously build their systems upon fancied *real* recognitions of past sensations in so-called present representations of such. The truth is that in the absolute sense we do not any more recognize sensations in their image representations than we recognize real things in their sensational representations. Until it dawned upon the human mind that its former so-called recognitions of an outer world could all be explained without the real existence of such a world, no one suspected the *reality* and *validity* of these recognitions; we now all admit such so-called recognitions to be but psychic processes; the *validity* of these processes and recognitions is, and we think must for a long time be, a subject of debate. We here wish only to point out that these parallel recognitions, so-called, of former sensations are likewise but psychic processes, the validity of which is as much open to suspicion, as inferred and as hypothetical as that of the so-called recognitions of a real world, and, indeed, vastly more so; for how commonly are our most confident memories mistaken, and our insane and hypnotic subjects engulfed in hallucination.

Still more is this truth forced upon us when we comprehend the details of these processes of so-called recognition; when we clearly understand the psychological difference between imagination and so-called recognition. If every one of us through life were but rational every alternate minute and insane turn and turn about every other minute, there would be no difference between imagination and reality. The grounds for our present belief in some real difference lies in the constancy of our belief itself, and when we come to examine into it, we find this belief is a hypothesis, an inference, and no *positive* knowledge. But what then are the grounds for this hypothesis? Plainly not in any simple direct cognitive act or state. We have sure reason to believe that our ordinary so-called perception of time relation is not a peculiar disparate state, but an apperceptive process; and similar analysis, I think, discloses to us that recognition is a similar apperceptive process, and that imagination is still another such a process. The difference between imagination and recognition lies first in a marked difference in the character of the thoughts which form the objective process of the apperceptions, that is, to which the associated ideas are added in the two cases; and, secondly, in the character of these *added* or associated ideas or processes. And as it is the nature of apperceived associations to be of like character to the objects of apperception, we shall find that the difference between the associated ideas in our two cases corresponds in characteristics to the original difference between the objective processes themselves. What then is this fundamental difference between imagination and reality? We can only answer with an hypothesis, and this hypothesis is, that all things *do* occur in a fixed order, that all occurrence *is* a fixed order; that not all this occurrence is perceived by us; that certain of the total occurrences of the universe result in fixed and definite influences upon our brain organism; that like causes produce like results; that like stimulations produce like sensations; that like series of stimulations are followed by like series of sensations; that these physical stimulations *are* alike and these corresponding sensations *are* alike, though the mere occurrence of their likeness by no means constitutes our recognition of this likeness; that owing to the peculiar nature of our physical organism and particularly of our central nervous organism, whereby physical processes tend to repeat themselves, certain representations or repetitions of sensations corresponding to these processes in certain characteristics *do* tend to occur whenever these physical processes *do* repeat themselves; that the accuracy and scope of complexity of temporal correspondence between these representative processes and their originals depends entirely upon the habit validity of these physical reiterative processes; that our recognition of this validity and correspondence does not consist in some super-added cognitive act over and above those psychic processes which correspond to these reiterated physical processes, *but is entirely dependent upon, and to be explained by, a hypothetically actual correspondence or likeness of these reiterative processes, both phy-*

sical and psychical, to former processes, physical and psychical; finally, and again, that not even the mere validity of this correspondence alone comprises "recognition," but that recognition is a psychological process, the validity of which rests upon the validity of such correspondence. Our hypothesis is that the events of our lives do happen in a single definite actual order, which so impresses itself upon our memory organism, that by proper associative incitement, this order tends actually to be repeated. It is true that this same memory organism, lacking these major associative incitements, forms secondary associations, and these tertiary, and so on almost to infinity; and in proportion to the frequency in which these minor associations occur, and in proportion to their kinship to original occurrences, do they also tend to rise in association processes. These minor and less constant associations are the basis of imagination; "imagination" is a word which we associate with these *inconstant* flights of association; "reality," "actual," are words we associate with the main *constantly* reappearing stream of association. The fundamental difference between imagination and recognition lies in the fact that the iterative habit of our nervous organism is so susceptible to original *outer* influences and so accurate and persistent in repeating these, that they ever *do* remain a comparatively unbroken series in representation, while those series which happen not by any outward actual order of incitement, but by secondary associations of portions of those primary series, do *not* persist in like unbroken representation. If, by any chance, a new link can be fastened into the original or actual memory order with the same associative firmness and strength as an actual occurrence would have been, then such will actually appear to be recognized as actually having occurred and psychologically will be so "recognized." Liars who frequently, actively and consistently enough practice their imaginary associations, do eventually arrive at such psychological "recognitions;" all of us at times suffer such hallucinatory remembrances, and actually believe we did so, or so, or that such and such happened, when, actually, they did not; and the hallucinations of the insane and the hypnotic are confirmative of our hypothesis. Imagination is inconstant memory; remembrance is constant memory. As we have said both these processes commonly go on without conscious recognition of the fact that we *are imagining* or *are recognizing*. When these last processes occur, simply the bodily act rises to the focus of apperception; and in apperceiving the "act of imagination," imaginative ideas, that is, inconstant memories are called up and flit before the mind; while in apperception of the "act of recognition," portions of the constant train of memory are called up to constitute the apperceptive association.

Let us summarize the foregoing: Our simple creature received series of like sensations, but he did not recognize them to be alike. So we, if incapable of memory, should experience often repeated sensations, but should never recognize them to be the same. Even, if endowed with memory, we should never recognize a constant actual series of life's events, did not life's events happen in a single definite order. Our actual remembrances are representations which *do* follow the actual order of original events. Our imaginations are representations which do *not* follow the original order. The validity of our imaginations and of our recognitions, depend alike and absolutely upon the degree of faithfulness with which the neural processes which produce them correspond to the neural processes which produced the original psychic events.

Briefly stated, the final result of this protracted investigation of the time problem is as follows:

The general consensus of past and of current opinion is that time perception must alone be accounted for within some peculiar simultaneous psychic state, and, according to most authors, by some peculiar and disparate form of consciousness, in addition to our stream of ordinary sensations and their representative images.

The conclusion which we offer is that the processes of our environment, of our bodily organism, and of the sensations and images which correspond thereto, are, in themselves, within the limits of the insoluble mystery of the existence of any physical or psychical existence at all, a sufficient explanation of time-psychology, and that time perception cannot be explained by any single state or disparate sense, but can alone be accounted for as a *process*. The bearing of the experiments of Section III upon these conclusions, and of the conclusions upon the experiments is obvious. The author is conscious that neither the one nor the other exhausts the topic, and will be content if they draw closer attention and study to the *habit relations* between neural and psychic PROCESSES.

Approved as a Thesis for the Degree of Doctor of Philosophy in Psychology at Clark University.

G. STANLEY HALL.

Worcester, Mass.,
Friday, May 1st, 1891.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

Report of six lectures on cerebral localization, delivered in Boston, by DR. HENRY H. DONALDSON, before the Boston Medico-Psychological Society, February and March, 1891. From notes by T. L. Bolton.

It was the aim of these lectures to show the bearing of the more recent anatomical investigations on the question of cerebral localization, rather than to give a full account of the subject.

LECTURE I.

Gentlemen:—I shall open this course with a statement of some recent advances in our knowledge concerning the structure of nerve cells and nerve fibers, and the relation of these to one another. The advance has come about by the introduction of a new method, which is due to the Italian histologist, Golgi.[1] To the labors of Golgi and his Spanish pupil, Ramon y Cajal,[2] is due the discovery of the most important points which are to be described. As everything depends upon the validity of the method employed, I will briefly state its essential character. The method of Golgi outlines the nerve cell and its prolongations by means of a deposit or precipitate which is formed just outside of these structures, and occupies the lymph space which surrounds them. The deposit is an inorganic precipitate of a silver or mercury salt, and forms a dense incrustation about the nerve elements. Further details are not necessary here. The result of this reaction is to outline the nerve elements in black on a light background. The inference is that, where this incrustation goes there goes a prolongation of the cell. On this assumption, which appears in the main well founded, depends the entire significance of the method. Golgi's first result was that the axis-cylinder process from the nerve cells was branched. Closer examination of axis-cylinder processes indicated that they might be grouped in two classes; first, those in which the branching was not sufficient to obscure the identity of the main prolongation; second, those in which the main prolongation divided into many branches soon after leaving the cell, and thus lost its identity and faded out. In the dorsal cornua and in the so-called sensory regions of the cerebral hemispheres, this second type of cells was found whereas the first type appeared in the ventral cornua of the cord, and in non-sensory portions of the cortex. From this general distribution, Golgi was led to designate the cells of the first type as motor and those of the second as sensory. To the axis-cylinder of the second type Ramon has added some very suggestive details in that he finds the smallest branches of these prolongations surrounding in certain cases cells, *e. g.*, the cells of Purkinje in the cerebellum, and enclosing them like a basket. This manner of termination of the ultimate branches of the axis-cylinder appears particularly well developed in the instance cited, but it furthermore appears to be the usual manner in which such branches end when they terminate in the neighborhood of nerve cells.

Besides the prolongations which come from nerve cells lying within the central nervous system, the dorsal cornua of the spinal cord are filled with a mesh-work of a similar character, which is due to a breaking up of the axis-cylinders of the dorsal roots. All the sensory fibers of the central system appear to come from the spinal ganglia or homologous structures,—the fibers of the optic nerve are alone the possible exceptions to this rule. It will be seen from this arrangement that the sensory cell of the older histologists—meaning, thereby, a cell situated in the dorsal cornua and sending an axis-cylinder through the dorsal nerve roots to the periphery—finds no place. It has, therefore, been thought best to modify the terminology so that by sensory cells are meant those forming the spinal ganglia and giving origin directly to the sensory fibers. Motor, or efferent, is the terms retained for Golgi's cells of the first type; but the cells of the second type, which he termed sensory, are perhaps better designated as central.

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[1] Golgi: Sulla Fina Anatomia degli Organi Centrale del Sistema Nervoso, 1886.

[2] A. Kölliker, Zeitschr f. Wissen, Zool. Band 49 u. 51.

While our interest has been specially attracted to the axis-cylinder process, it may be well to point out that the protoplasmic prolongations of the nerve cells have been brought out by this method with unusual distinctness and detail, and since they cannot be seen to unite with one another nor to give rise to nerve fibers, the question has been raised concerning their function, and the general conclusion is that they must be looked upon as nutritive. I wish for a moment to leave the nerve cell and to take up some recent results relating to the histology of nerve fibers. We have in the medullated nerve fiber an axis-cylinder surrounded by a sheath of somewhat complex structure. The axis-cylinder is the portion of the fiber which interests us at this moment. There are, roughly speaking, two views concerning its structure. One which has been ably advocated by Nansen[1] looks upon the axis-cylinder as a mesh-work, in the cavities of which is to be found a plasma, and this plasma is the active substance in process of conduction. The second view, which has recently been elaborated by Boveri,[2] considers the axis-cylinders as made up of a number of fibrillæ floating in a plasma. These fibrillæ are considered as unbranched and as continuous from the nerve cell to the termination of the axis-cylinder process to which the cell gives rise. If we look upon the axis-cylinder prolongation as made up of these fibrillæ, then the branching of the axis-cylinder is to be interpreted as the giving off of small bundles of fibrillæ. In considering the manner in which the nerve fibers arise from the nerve cell, I may allude to the recent observations of His[3] on the development of the spinal ganglia in man, which show that the well-known T process of Ranvier, by which the cells of the spinal ganglia are joined with the nerve fiber, is a derived structure. These cells originate as bipolar nerve cells, similar to such as are found in lower vertebrates. In the course of development, however, the two poles from which the nerve fibers originate gradually approach and finally fuse with one another, thus giving rise to the stem of the T, one branch of which runs centrally and the other toward the periphery. In the hands of Ramon the methods of Golgi applied to fœtal, or very young animals, in which the nerve fibers were only in part medullated, has developed some startling results concerning the branches of nerve fibers. The net work which Golgi observed in the dorsal cornua of the spinal cord was the final termination of the dorsal root fibers. Ramon has now shown that as soon as the fiber enters the cord it divides into two branches, one running cephalad, the other caudad. That these two branches give off further, branches at right angles to their course, which have been called collateral fibers, and that it is the termination of these collaterals which gives use to the mesh-work already mentioned. These collaterals, however, are not confined to this group of fibers alone, but, as it appears, may be found arising from fibers in almost any part of the cord. This rather startling result is difficult to explain, if we consider that the medullary sheath of the medullated fibers in the cord is unbroken throughout its extent. The recent observations of Porter[1] show, however, that there are nodes of Ranvier in these fibers in the spinal cord, and, although we know nothing of these nodes further than their probable existence, they would seem to offer a convenient point of departure for the collateral fibers and thus bring the law of branching in the central system into harmony with that for the peripheral nerves, where the branches occur at a node of Ranvier.

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[1] The structure of the Histological Elements of the Central Nervous System. Bergen, 1887.

[2] Abhandl. d. k. bayer, d. Akademie. Wissen, Band XV, 1885.

[3] Archiv. f. Anat. u. Physiol., 1890.

There is one principal point which has thus far been left out of the discussion, viz.: how far are the fibers which are brought out by this method of Golgi to be identified with the medullated fibers with which we are commonly familiar? It seems beyond doubt that a number of the structures thus developed are unmedullated even in the adult. One important piece of evidence has been presented by Flechsig[2]. He has succeeded in staining specimens of nerve cells, which had been previously treated by Golgi's method, with a dye stuff which stained medullary substance red. According to this result he finds many of the branches of the axis-cylinder process to be medullated, and thus it would appear that these branches may become medullated nerve fibers.

We come now to the final point of the connection between nerve cells and nerve fibers. Of course each nerve fiber is looked upon as the outgrowth of a certain nerve cell, and the connection referred to in this instance is that between the termination of a nerve fiber and neighboring nerve cells with which it may be supposed to be physiologically associated. The connection of the nerve cells with one another is but another aspect of the same problem. It may be stated as a general result that this method fails to show any direct continuity between any prolongations of one nerve cell and those of another. This lack of demonstrable continuity has led to several hypotheses; the best of which is perhaps that of His, who points out that, however closely the protoplasmic prolongations may be interwoven, there is always a somewhat between them which we are accustomed to designate as ground substance, and to this ground substance must be attributed the function of establishing continuity between the nervous elements. The earlier workers in this line had directed their attention to the remarkable branches of the axis-cylinder prolongations, and thought in some way these would account for the physiological connections between cells. At present, however, there is no positive evidence in favor of such a view.

Taking a general view of the nervous system, we find the sensory impulse coming in through the dorsal roots which form a mesh work in all probability connecting, as a rule, with some of the central cells, and thus finding its way to the higher centers, or perhaps without the intermediation of central cells, reaching the efferent cells in the ventral cornua. Nansen, who wrote a few years since and was much impressed with the possibilities of the network formed by the prolongations of the axis-cylinder, drew up a scheme which I believe has been received with some favor, according to which the nervous and mental process were considered as taking place in this mesh work, while the cells were regarded as having a nutritive function and acting as the supporters of the mesh work alone. It now remains to point out some of the peculiarities of our point of view as determined by these results. I will give them in briefest form posssible. The new methods show that the axis-cylinder is branched; that there are several types of axis-cylinders; that these branches may become medullated, hence several fibers may arise from one cell; that there are no sensory cells in the central system; that the axis cylinders are made up of fibrillæ; that nodes of Ranvier occur within the central system; that collateral fibers arise from the longitudinal fibers of the spinal cord; that no nerve fibers come from the proto-plasmic process; and that there is no continuity between cell elements in the central system, but that probably physiological connection is dependent upon peculiarities of the ground substance which surrounds them all.

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[1] Quart. Journ. Micros. Sci. Feb. 1890.
[2] Archiv. f. Anat. u. Physiol. 1889.

LECTURE II.

Gentlemen: This lecture will be a continuation of the preceding upon the architecture of the nervous system. I shall consider the size of the nervous elements, their numerical relations, the relation of the cerebral cortex to the optic thalamus, and say a few words upon some methods of ascertaining the localization of functions in the central nervous system. Nervous elements differ much in size. In speaking of the size of the nervous elements, one point should be emphasized. Nerve cells and fibers are in reality but parts of the same structure; the fiber is a branch of the cell. The nervous system must, therefore, be considered as composed of one kind of elements—the cells. There is a relation between the size of the cells and that of the fibers, more especially between the nucleus of the cells and the fibers.[1] The nerve fiber is composed of a sheath and an axis-cylinder. This axis-cylinder is made up of a number of minute fibers known as fibrillæ. The branching of a nerve is simply the separation from the main axis of a number of fibrillæ surrounded by a continuation of the sheath. All nerves terminate finally in a mesh-work of the fibrillæ. If we examine the ischiadic nerve of a frog, we find it to be conical in shape. Transverse sections of the nerve at the hip, the knee and the ankle, show a diminution in the diameter of the individual nerve fibers, as we proceed from hip to ankle. The old explanation for this was that the fibers themselves were conical in shape; but this is probably incorrect. The diminution in diameter is really due to the fact that the nerve trunk branches and the branches from the higher levels contain the fibers of greatest diameter, and those from lower levels contain those of smaller diameter. The physiological bearing of this is important. Suppose the proximal muscles of a limb to be as richly supplied with nerve fibrillæ as the distal muscles are, then since we have found that nerve fibers of large diameter supply the proximal muscles, it follows that in order to have the same abundance of fibrillæ for the distal muscles, that the individual nerve fibers supplying them must be more numerous. Here let us depart from the main point for a moment in order to bring up a subject necessary to make clear the explanation that follows. Every motor cell, so far as is known, acts as a unite and give rise to but a single motor fiber. Thus, whenever a discharge occurs in the cells, the muscle in which the fiber terminates must respond. If, however, a muscle be the termination of several fibers, several cells control its action and thus a finer control is brought about. The significance of size of nerve fibers is usually stated to be that the larger fibers run the longer course, for larger fibers are necessary to carry the nervous impulses to greater distances, on the analogy of electricity where the larger wire is the better conductor. The largest fibers in the frog arise from the lumbar region of the spinal cord and terminate in proximal muscles. The differentiation of function is slighest here, and fineness of adjustment is least needed. From this we conclude that large fibers are concerned with coarse adjustments, and fine fibers with fine adjustments. We turn now to consider what is known of the numerical relations within the central nervous system. Birge,[1] working with Gaule, undertook an actual count of the number of the fibers entering the spinal cord by the anterior and posterior nerve roots. This actual number of motor fibers determined by the count corresponded very closely with the number of cells in the ventral cornua of the spinal cord. From this a munerical equivalence between motor cells and motor fibers was inferred. Gaule[1] has carried the investigation a step further by counting the number of nerve fibers in cross sections of the spinal cord at five different levels and has then attempted to determine whether there was any relation between the number of fibers of these various levels and the number of the dorsal root fibers entering the cord at the same levels. He finds as a result that each root fiber calls for eleven fibers in the cross section. Thus there appears to be a distinct numerical relation in this instance. It should be remembered that Gaule is dealing with medullated fibers only and whatever relations may be dependent upon unmedullated fibers do not enter into his calculations.

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[1] Mason; Journ. of Nervous and Mental Disease, 1880,-81,-82.

In his paper, upon this subject, Gaule has a system of philosophy which is peculiarly his own.

It may be roughly stated as follows: As the cells are composed of molecules which are made up of atoms standing in a fixed and definite relation to one another, so the body is composed of cells which appear in fixed proportion, for instance every sensory fiber demands eleven nerve fibers in a cross section of the cord.

Charts of the brain have been made by several authors for the purpose of showing the various developments of the cerebrum. Broca, Obersteiner, Eberstaller and Wilder have constructed such charts as you see, according, as it was their purpose to illustrate particular points. The need for some definite diagram on which to plot lesions useful for the localization of function has been felt. For this purpose the best of these perhaps is that of Eberstaller, which was designed to show almost every detail, while that of Wilder brings out the early developed characteristics mainly. The amount of variation that may occur in the central nervous system is very great. All brains differ. Several attempts have been made to measure the extent of the gray substance of the hemispheres. The figures vary between 1800—2700 sq. cm. The important relation is that the sunken gray matter lying in the sulci is about twice that which is exposed on the surface. Suppose that in brains which are comparable, the sulci have a similar depth and the nerve elements a similar size, then a richly convoluted brain would contain a greater expanse of gray matter, and a greater number of cells would be found in such a brain. The converse would be true; a poorly convoluted brain would contain a less expanse of gray matter and a less number of cells. If, as Gaule asserts, every nerve cell gives rise to a definite number of fibers and the amount of branching be similar in cases compared, that brain which contains the most cells, has the most branches. These branches become medullated fibers and constitute the white substance. According as the number of cells is greater, and hence the number of fibers large, the amount of white substance will be great and the size of the brain increased. However, Gaule's numerical relation is probably only partly true. All of us are acquainted with large brains that are poorly convoluted, and the preceding remarks on the relation of the size of the cerebrum to the abundances of their convolutions have for their main purpose to direct attention to the compensatory developments which probably occur there, but about which we know almost nothing.

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[1] Archiv. f. Anat. u. Physiol., 1882.

[2] Abhandl. d. König. Sächs. Gesell. d. Wissens. B. XV, 1889.

Such a scheme as that of Gaule at once raises the question, how far variation may occur within the nervous system. We have there many decussations, such as that of the optic tracts, of the cranial nerves and of the pyramids. Variations in these decussations are known to all. Our physiological inferences are based upon anatomy. If the anatomical foundation can vary, it is a most important point, especially in the case of the nervous system, and one which must always be kept in mind, when physiology and anatomy appear to conflict.

It may be possible sometime to track a sensory nervous impulse from the periphery to the contex. All of us have been taught that sensory fibers enter the spinal cord by the dorsal roots, and proceed by the dorsal columns towards the brain, and terminates in the ganglia of these columns. From these ganglia they pass to the thalamus of the opposite side, by way of the lemniscus. The cells of the thalamus are connected with the cells of the cortex by the fibers of the corona. There is some reason to think that whatever the source of the sensory impulse, it must pass through the thalamus before going to the cortex. Several attempts have been made to determine the relation between the portions of the cerebral cortex and the thalamus.

Monakow[1] determined this relation in rabbits. He operated on the dorsal and lateral surface of the hemispheres only. Here the removal of definite portions of the cortex caused an atrophy in an equally definite portion of the thalamus. There were certain portions of the thalamsus, which were not affected by any of the lesions and these by exclusion may be supposed to be connected with those portions of the cortex which were never injured. These results have been in part verified for man.

LECTURE III.

Gentlemen: We shall consider in this lecture the motor regions of the brain. The middle portions of both hemispheres contain motor centres. A history of the subject is not needed; but the method used for the discovery of these centres and of the refinement of their subdivisions deserves some attention. Our especial interest is in the subdivisions. Motor is not a good term to apply to this region, but it is the best we have. The idea of the motor centres from an anatomical point of view is useful. The central nervous system may be considered as a conical mass with the cortical centres in the base of the cone, the apex of the cone representing the spinal cord. A nervous impulse proceeds from the periphery toward the cortical centres and at various levels in the cone it encounters masses of gray matter which increase the possible number of paths the impulse may take. The important question then is: Does the impulse diffuse itself throughout the entire system or follow a fixed path to a definite centre in the cortex? The possible paths depend upon the complication of the central system. In higher animals, the possibilities are many. The paths followed in any given case depend upon physiology rather than upon anatomy. The path of the impulse appears to be simple; it starts from a small area in the periphery and reaches a small area in the cortex. The cortical centre is but a specific point in the path of the impulse, where the impulse turns to pass centrifugally. As points in the path of an impulse the cortical centres are like innumerable other points in the central system, but they have a peculiar interest because of their great accessibility and because they are in a region which is supposed to be associated with mental phenomena.

Let us now consider the method of stimulating the cortical centres. Horsley[2] attempted to determine whether all the parts of the muscle curve given by a muscle during an epileptic seizure, were due to the cortex alone or to cells lying outside of the cortex. He exposed the brain of a monkey and found the area for the control of the leg. An electric current was applied and an impulse sent to the spinal cord. By tapping the spinal cord in the dorsal portion and recording the impulses passing there in the pyramidal tract, by means of an electrometer, both the tonic and clonic portions of the curve were shown to be due to the impulse from the cortex. Others have observed that when the cortex was excised, and the stimulus applied directly to the nerve fibers, the clonic portion of the curve dropped out. This fact has been used clinically. Horsley has studied the minute representations in the cortex of movements of the head and limbs. His method was to apply to the cortex electrodes, two mm. apart, with a current just sufficiently strong to bring about a contraction. The strength of the current was important; a weak current would contract few muscles slightly and a stronger one would cause a stronger contraction of a greater number of muscles. To explain this contraction in the last case a slight irradiation of the stimulus was supposed to take place, so that neighboring centres were involved. The diagrams show the results of Horsley's experiments. The outline of the region enclosing the motor areas is largely bounded by fissures—below by the Sylvian fissure, behind by the inter-parietal, in front it passes somewhat in front of the praecentral, and above the margin of the hemisphere forms the boundary from this point of view. There is no necessary connection between areas and sulci; some areas appear to be limited by sulci, and others not. The portion of the cortex lying in the sulci is one that usually escapes stimulation.

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[1] Arch. f. Psychiatrie B. XII, 1882.

[2] Gotch and Horsley—Proc. Roy. Soc. London, 1888, XLV.

In the motor region of the monkey's brain the motor areas for the control of the various parts of the body were found to be located thus: The head and eye area is located in the front part of the motor region; above the Sylvian fissure is an area for the control of the larynx, pharynx, and the movements of the mouth and face; back of the head area and above that for the face is an area for the upper limb; still back and above this is an area for the control of the lower limb; and between the areas for the upper and lower limbs is one for the trunk. It will be noticed that we thus pass in serial order through the centres for the head, arm, trunk and leg, the first most anterior and the last most posterior. The same serial arrangement is maintained on the mesal surface. There is a remarkable independence between the size of the centres and the mucles they control. The centres controlling the head and face constitute about half the motor region. Where the muscles are large and the movements crude, the representation in the cortex is small. The area for the head has been longest undergoing differentiation; next to this in order of development is that for the upper limb, that for the lower limb, and lastly for the trunk. Beevor and Horsley[1] have studied in detail the anatomy of these areas, especially that of the arm, and have found there is a subdivision of function within them. They divided the area of the arm into squares of 2 mm. on a side and stimulated these squares in regular order. Attention was given to what movements came out first—the so-called primary movements. The first movement following a given stimulus in the uppermost part of the arm area was the movement of the shoulder; when the stimulus was applied a little lower down, a movement of the elbow took place; it was then applied further down still, and a movement of the wrist was the result; when however, the stimulus was applied to the lowermost part of the arm area, the thumb responded. The centres for the control of the shoulder and the thumb are then farthest separated. The thumb is the most highly modified portions of the upper limb, and its movement is the most highly modified movement. The opposition of the thumb is very widely represented. It was determined that in general the march of a spasm affecting the arm follows the order of the centres within the arm area; that is, if the spasm starts at the shoulder, it passes by regular progression to the fingers, or if it starts in the thumb, it passes in regular progression along the limb in the opposite direction. It appears, however, that the connection between the thumb and shoulder is very slight, so that a spasm starting in the shoulder does not usually terminate in the thumb, nor does one commencing in the thumb, terminate in the shoulder. Looked at from one point of view this would appear to indicate that the association tracts in such an area were short, and, as a rule did not extend beyond the nearest lying centre.

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[1] Beevor and Horsley—Phil. Trans. Roy. Soc. London, 1887, 1888.

No centre will give a particular movement exclusively, but certain movements usually follow stimulation in a definite portion. The subdivision of function in other areas was studied by these same authors. They endeavored to determine the kind of motion resulting from stimulation. They found that extension of the arm followed stimulation in the upper portion of the arm area, flexion in the lower portion, and that there was a confusion of movement, when the stimulus was applied to the central portion of the area. Extension and flexion in all centres are usually widely represented. Let us carry these facts over to the diagram of the mesal surface of the human brain[1]. The localization of functions on the mesal surface appears in the order of head, arm, trunk and leg from before backwards. The basis for the schematic representation in man is about as follows: some of it is based upon analogy and some on the results of direct stimulation. Below and behind the head and eyes there is an area for the control of the face. The localization of the face area is based primarily upon pathological and clinical evidence and secondly upon analogy with some experimental evidence for its support. The face area can be broken up into a number of other centres. The muscles concerned in speech are represented in this area. The speech centre appears to be a duplication of this representation in a refined form and is usually left out of the discussion of motor centres. It is, however, a motor centre, having fibers which pass into the internal capsule. We know nothing of the subdivisions in it. Has this motor region other than motor functions? Motor reactions follow the stimulation of the cortex outside of what is generally designated as the motor region. Stimulation in the occipital region and in the tip of the temporal lobe gave motor reactions. When the stimulus was applied to the tip of the temporal lobe, reactions in the mouth and nose followed. Movements of the eyes were to be obtained by stimulation of the head area and also of the occipital region of the cortex. These motor reactions were due, according to Ferrier, to the stimulation of the sensory cells in the cortex which in turn reacted so as to bring about movements. Schäfer[2] carried the investigation further.

When he applied the stimulus to the occipital portion of the cortex and produced movements of the head and eyes, he observed that the reaction time was longer, than when the stimulus was applied directly to the head area. By cutting out the proper centre in the head area and then stimulating the occipital portion, reaction was still obtained, which showed that motion in this case was independent of centres in this area. This is but one example of apparent multiple representation of movement in the cortex.

LECTURE IV.

Gentlemen: We shall consider this evening the sensory centres. The motor centres form a dividing line in the cortex, behind which lie the sensory centres, and in front is an unoccupied area which is left out of the discussion. The anatomy of the sensory region needs some attention. If we section the white matter in the motor region, degeneration follows both toward the thalamus and toward the cortical cells. The same holds for the sensory region. These fibers are, therefore, arranged to carry impulses both ways, that is, there are both afferent and efferent fibers. Ferrier began the study of the sensory centres. By stimulating the sensory centres he was able to produce motion, but the motion was particularly associated with the peripheral sense organs. Permit me to call attention to a peculiarity of the sensory region. In the lower animals sensation is not so accurately located as motion. The reactions we have to study are crude. Slight loss of sensation can not be shown. The results of the study are often contradictory and no reconciliation seems at times possible. All the results that are accumulated are not of equal value. I shall make use of some of those results which appear most trustworthy. When one set of experiments support a view with good positive evidence, and the opposite view is sustained by equally good positive evidence, there is reason to think that a further extension of the hypothesis will harmonize the views.

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[1] Mills. Trans. Cong. Am. Phys. and Surgeons, 1888.
[2] Schäfer: Internat. Monatschr. f. Anat. u. Physiol. Leipz., 1888.

Let us take up first the cortical centres for vision. The experiments have been made upon monkeys. In them we have the occipital lobe and the angular gyrus as cortical centres for vision. In man the cuneus is supposed to be connected with vision. The discussion that has taken place has been concerned with the relative values of the occipital lobe and the angular gyrus as centres for vision. The evidence is this: Brown and Schäfer[1] removed the occipital lobe on the left side and the result was a defect of vision in the left halves of the two retinae. This result was persistent, no recovery of vision occurred. They then removed the occipital lobes on both sides and complete blindness resulted. Again the results were permanent. Ferrier criticises this on the ground that they injured the angular gyrus. Brown and Schäfer removed the angular gyrus on one side and no permanent defect of vision followed; even when they removed it upon both sides, the defect was not permanent. The criticism that is made here is that they gave attention simply to permanent defects. The removal of the angular gyrus caused transient blindness in the opposite eye. The animal could see objects afar off but could not see them so well near to, and this was apparently a persistent symptom. Ferrier[2] removed the occipital lobe and found no disturbance of vision on either side. However he left the ventral portion of the lobe intact and this is used as an argument against his results. When he removed the angular gyrus, the opposite eye was affected transciently; but vision returned after a time. When the other gyrus was removed, the other eye was very much affected, and the eye upon the same side slightly so. He did not get a blind monkey from his operations upon either the angular gyrus or the occipital lobes alone. When he removed both the occipital lobes and angular gyri, his monkey became permanently blind. The removal of the occipital lobe from which he got little or no effect, and of the angular gyrus from which he got transcient effects, when each was removed separately, gave permanent and real blindness, when removed together (!). Transient symptoms in lower animals may become permanent in higher animals. Becterew[3] has published an account of his experiments upon dogs and rabbits. With the removal of the occipital lobe, hemiopia occurred. The removal of the angular gyrus produced amblyopia in the opposite eye. We pass now to the clinical evidence in man. The region of the cuneus is usually described as the principal centre of vision. Reported lesions mainly occur in the apex of the cuneus and extend from the occipital lobe into the angular and supra-marginal gyri. Hemiopia follows lesions in the occipital lobe. In the neighborhood of the angular gyrus, lesion produces crossed amblyopia. The same relations hold in man as in monkeys. Some subdivision of the optic centre has been made out. If the lower portion of the occipital lobe in the monkey be stimulated an upward movement of the eyes is produced. Stimulation in the middle portion causes lateral movements and in the uppermost portion downward movements. (Schäfer[1]) This has been interpreted to mean a detailed projection of the retinae upon the cortex. A partial decussation of the optic tracts would account for the location of half of each retina in each occipital lobe. Some clinical evidence for sub-division in man has been presented (Hun. Amer. Jour. Med. Sci. Jan., 1887.), but is at present insufficient.

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[1] Phil. Trans. Roy. Soc. London, 1888,

[2] Lancet. June and July, 1890.

[3] Neurologische Centralblatt, No. 8, 1890.

Among birds a partial decussation of optic tracts has not been observed anatomically. In owls, however, it would seem to occur. Ferrier found that when the occipital lobe was removed, the eye of the opposite side became blind to tests applied. For purposes of experiment the sound eye was first completely bandaged; it was next enucleated, when the blind (?) eye showed enough sensitiveness to enable the bird to catch a mouse in its cage. It would appear that the small sensation here was inhibited by the simple presence of the sound eye. The inference then is that there is a partial decussation in this bird. From our present point of view, hemiopia is dependent upon partial decussation and partial decussation must be supposed to exist, when hemiopia occurs. We now pass to the auditory sense and its location. Here the positions held by the experimenters are at present irreconcilable. Ferrier stimulated the posterior portion of the superior temporal gyrus in monkeys and got a movement of the ear. Excision of this gyrus produced deafness, when the excision was made upon both sides. Brown and Schäfer removed this region and deafness did not follow. In man the clinical evidence favors Ferrier. There are two cases at least where lesion in the posterior portion of the superior temporal gyri caused complete deafness in man. In the auditory form of aphasia this region is undoubted the auditory centre.

Taste and smell are of but little importance in this connection. In this case stimulation indicated the tip of the temporal lobe as their probable centres. If, as we suppose, the discriminative use of an organ determines its representation in the cortex, these centres would then have small cortical representation. The same disagreement exists here among the investigators. Ferrier finds lesions here to produce a loss of taste and smell; Brown and Schäfer find the opposite results. Let us consider now the cutaneous sensibility, and here experimentalists are in accord upon the main issue. Ferrier produced a disturbance of cutaneous sensations by the removal of the hippocampal gyri. Horsley and Schäfer[2] followed up these results and removed the gyrus fornicatus in monkeys, when hemi-anæsthesia of the opposite side of the body followed. Partial removal was tried, but the disturbance of sensibility to pain and tactile stimuli for different segments of the body was not localized. This cortical centre appears to be connected, mainly, but not entirely with the opposite side of the body. The symptoms of allochiria followed lesion on one side. The animals thus operated upon appeared to recover after a time. The degeneration following this lesion in monkeys has been studied very carefully by France.[3] In the internal capsule it could not be definitely located, but in the crura and the pyramidal tracts the location was clearly made out. The important point here is the degeneration of sensory fibers downward, and their presence in the pyramidal tracts in the cord and further the observation that they occupy a definite position in these tracts. France appears to have guarded against any confusion of the lesion described with the lesions following injury to motor centres. Nothing can be said at present of the frontal and ventral portions of the cortex, so we next pass to the localization of lesions in aphasia. Starr[1] gives three periods in the development of our knowledge of aphasia. The first is that of Broca-motor asphasia—the second, that of Wernicke—sensory aphasia—and the third, that of Charcot—a further analysis of sensory aphasia. According to Charcot the idea of an object for the educated man is dependent upon two sensory centres—auditory and visual, and is capable of expression either by the spoken or written word. We have then four principal centres to consider. In visual and auditory aphasia lesions occupy definite areas. For motor aphasia the motor centre is the speech centre. Sensory lesions occur mainly in the region behind the fissure of Sylvius; auditory disturbances being associated with lesions of the superior temporal gyrus, and visual disturbance with those of the angular gyrus. Where the motor centre for writing may be, is not clear. It does not appear, however, to be within the arm area and may possibly hold a relation to this area similar to that which the speech centre holds to the face area. The connection between the sensory and motor centres involved is probably made by association fibers which pass beneath the island of Reil. The sensory form of aphasia is capable of very considerable subdivision, and seems destined to yield results of much psychological importance. I would call attention to the fact that even Charcot's scheme is capable of extension and that aphasia or its intellectual equivalent would be in a deaf mute a lesion in the pathway formed between the centres for cutaneous sensibility, and that for the movement of the fingers in the arm area. That in other words any sensory centre may form the first link and any motor centre the second, and with this may be associated the intellectual life of the individual.

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[1] Proc. Roy. Soc. London, Vol. 43, 1888.
[2] Phil. Trans. Roy. Soc. London, 1888.
[3] Phil. Trans. Roy. Soc. London, 1889.

Of the processes occurring in nervous system, none perhaps contribute more to our anatomical information than that of degeneration. Degeneration is, however, a very complicated process. In the higher animals a section of nerve fibers within the central nervous system is not followed by reunion. The nature of the degeneration which follows such a section is dependent on a large number of conditions. To take an example which is related to the question of the representation of the cortex in the thalamus which we have just discussed: If in the rabbit the motor fibers coming from the cortex be sectioned in the crura, the distal portion alone degenerates. If the section of these same fibers be made between the internal capsule and the cortex, not only the distal but the proximal portion with its associated cell degenerate. The reason for this is by no means clear, but may be dependent on the connection of this region with the thalamus. The peripheral sensory nerves furnish an example, where the direction of the nerve impulse and direction of degeneration are dissimilar. In a very young animal separation of a motor nerve at the point where it emerges from the central nervous system is often followed by complete absorption,[2] of both nerve and cell within the nervous axis. If a somewhat greater length of nerve be left attached to the central portion, atrophy only, and not absorption occurs.

Whether the portion within the central axis is absorbed, because, in the first instance, too much has been removed, or because the part removed had a special nutritional value from its position, must be left undecided. But the ultimate disappearance of the residual portion seems in some way to depend on the struggle for existence among the cell elements in the growing organism. The failure of nerve fibers to unite within the central nervous system might be thought to have some relation to that curious interdependence between growth and specialization by which the one is exclusive of the other. But on the whole it appears as though the conditions of nutrition would offer the best explanation for what occurs.

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[1] Trans. Cong. Am. Phys. and Surg. Vol. I. 1888.
[2] Forel: Arch. of Psychiatrie B. XVIII, 1887.

LECTURE V.

Gentlemen: Permit me to call attention to certain facts which are somewhat aside from the direct line of the previous lectures. We have spoken as if cerebral localization were an absolute fact, and such is practically the case, when we confine our attention to man and the monkeys. If, however, we study cerebral localization in the vertebrate series, we find that it becomes less perfect as we pass downwards. In this matter, comparative anatomy is the starting point. I have here drawn the brains of a dog and of a bony fish. In these widely separated forms, the anatomists can identify the subdivisions of the encephalon, which are homologous. From the physiological side, the question of importance is, whether homologous portions of the nervous system have the same relative function throughout this series. Not many years ago, this question was answered in the affirmative. The experiments of Goltz upon dogs which led him to deny cerebral localization in man were based upon this assumption. We propose this evening to present evidence for the view that homologous portions of the nervous system have not the same relative function in the lower as in the higher vertebrates. The problem may be expressed in anatomical terms; if we attempt to depict the paths along which an impulse must pass from the time it enters until it leaves the central nervous system, we have some such scheme as this: A nerve fiber comes from the periphery, is interrupted by a cell in the spinal ganglion, and enters the cord by the dorsal cornua; it connects itself in some way with a central cell. This cell is in turn connected with a motor cell, from which a fiber passes out of the cord by the ventral root. An impulse thus enters by the sensory and passes out by the motor fiber. This pathway for an impulse occurs from one end of the nervous system to the other, and may be designated as the segmental pathway. On this segmental system, composed of the spinal ganglia, the central cells and those giving rise to the motor fibers, there is superposed a mass of material which is represented by the thalami and cerebral hemispheres. The exact relations of these two structures are not known. For our purpose, we may look upon them as forming a part of a long or central pathway, over which an incoming impulse may, in some cases, pass. It is necessary then to elaborate the diagram, and make it possible for a sensory impulse to pass by means of a central cell to the cortex, where, in all probability, another central cell is interpolated in its course; from there, it passes to the cell giving rise to efferent cortical fibers, and so back to the segmental motor nucleus. This latter is the long or central path, which may always be contrasted with the short or segmental path. The application of such a scheme to the question in hand is this: Where the segmental or short path is highly differentiated, we would expect but little control from the cerebral hemispheres; whereas, where the long or central path is highly differentiated, we would expect the function of the cerebral hemispheres to be important, and the segmental path unimportant.

To begin with a fundamental question: Are both paths always permeable? This has been tested by the stimulation of the cerebral hemispheres in different orders of vertebrates, in which operation a certain portion of the long path was made te conduct the impulse, and this portion was thus shown to be permeable. The inference drawn is that if permeable through a portion of its length, the path was permeable throughout its entire length. If we apply this test at different levels in the vertebrate series, we find, as a general result, that the long path is always permeable, and, at the same time, we find that the differentiation in the cerebral hemispheres as indicated by the specialized character of the response, decreases from the higher to the lower orders; further, that centres where they can be made out, are less clearly circumscribed and subdivided in lower as compared with higher forms. In the very lowest forms examined, like the bony fish, the reaction to stimulation of the cerebrum is so crude and generalized that it is not distinguishable from reactions obtained through the segmental system. To determine whether the segmental path is permeable, the central path must be destroyed and reactions of the animal then observed. In general, it is found that an interrelation exists between the short and long path, of such a nature that the high development of the one is associated with the low development of the other. In the very highest vertebrates, it appears that in most parts of the cerebral system, the segmental paths are not permeable; but in animals below the dog, they certainly are permeable, and the complexity of reaction of which they are capable increases as we pass down the series. Before entering upon a description of the disturbances following interference with the cortex in mammals, such as the dog, I wish to refer to one immediate consequence of the operation. When a portion of the cortex is removed, a considerable number of the conducting fibers, which remain, undergo a secondary degeneration. In the process of dying, these residual fibres must get rid of their energy, and, in so doing, they cannot fail to influence the portions with which they are connected. It is apparently due to this disturbance that the transient phenomena, which act like inhibitions, arise. Turning now to the special cases, we shall commence with the experiments upon dogs, and before we have finished, I shall hope to have presented evidence for the statement that in the vertebrate series at its lowest limit, sensation and motion, spontaneity and choice are independent of the cerebral hemispheres, but that the dependence of these functions upon the hemispheres increases as we ascend in the series.

This figure[1] represents the brain of a dog with the right hemisphere removed. The animal lived more than a year after the last operation. The senses of sight, hearing, smell and taste were more or less impaired. The animal was a stupid creature, but the disturbances of motion were not to be seen until the two sides of the body were compared, when it was noticeable that motion was impaired upon the side opposite to the lesion. In the second diagram is represented a case where the attempt was made to cut out the frontal portions of both hemispheres. In addition to the intended removal, a secondary degeneration of the left occipital region occurred, which left in the end hardly more than one quarter of the hemispheres intact. The dog lived two and a half months after the last operation, and exhibited that ceaseless activity characteristics of dogs from which the frontal lobes have been removed. Voluntarily, it did not take food; but when food was given it, all the mechanical processes of chewing and swallowing were executed. The emotional sounds—barking, whining, growling, etc.—were appropriately used. At the time, when this animal was described, the operation was the most severe recorded for dogs. Since then Goltz has made a complete removal of both hemispheres[2] and the animal lived 51 days after the operation. This individual, for one reason or another, preserved no special senses. There was, however, no paralysis of any muscles, and the dermal sensations were everywhere present.

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[1] Goltz Pflüger's Archiv., Bd., 42, 1888.
[2] Neurologische Centralblatt, May, 1889.

As in the previous case, it required to be fed, but food placed well back in its mouth was properly chewed and swallowed. It moved about spontaneously, would stand upon its hind legs and walk in a co-ordinated manner. When hungry, it was restless; when satisfied, it slept. It could be awakened by a touch upon any part of its body, and, when so roused, it stretched after the manner of waking animals. From the foregoing, it will be seen that the loss of the hemispheres in the case of a dog is a pretty serious matter, but that a dog is still capable of living after such an operation, and preserves at least his dermal sensibility and considerable control over his muscles. As regards the special senses, we can only say that some remnant of vision may remain in a dog thus operated upon.

Christiani[1] experimented upon rabbits, and showed that when both hemispheres are removed, the rabbit can still see and hear, and retains its dermal sensibility. His experiments are open to the objection that the animals were not kept alive for more than two days. The operations, however, were perfect, and observations began immediately after the operation was performed. A rabbit is less disturbed by the loss of its hemispheres than a dog, and it is particularly noticeable that hearing was retained. Birds also have been worked upon. Schrader[2] has done the best work upon pigeons. There is a wide difference in the intelligence and the relative value of the hemispheres among the various orders of birds. When a pigeon loses its hemispheres, it may at once begin to walk or will fly, when thrown into the air. In some cases they fall asleep, and are aroused only by hunger. Often, when placed upon the floor, such a bird will walk continuously until some obstacle stops it for a moment; when stopped, may fall asleep. The bird sees, and this spontaneity is due to the fact that it can see, for it roosts when it becomes dark. It can not feed itself, when its cerebral hemispheres are entirely removed, but the retention of a small portion will suffice for this purpose. It hears slightly, but will not heed the call of other birds. Taste and smell are difficult to demonstrate, even in normal birds. It will choose between two perches the one that is best suited to its purpose.

Reptiles have been experimented upon very little. We may pass at once, therefore, to frogs. A frog[3] is not deprived of all spontaneity, when the cerebral hemispheres alone are removed, that is, when the thalamus remains intact. The frog can jump and feed itself; it avoids obstacles and can see. It burys itself in the winter and awakes in the spring. In fact, the chief difference between the operated and normal frog is in a certain slowness and sluggishness in initiating any action. This discussion is complicated by the fact that the cerebral hemispheres consist of a basal ganglion, over which is spread a mantle. The mantle is functionally the more important in the higher animals, while it is of very little importance in the lowest vertebrates, and in the frog it may be removed without producing observable symptoms. In the bony fish, the mantle can be shown histologically to be non-nervous in structure, and whatever cerebral functions such an animal may have are associated with the basal ganglia. Cutting off all but the thalamus in a fish, there is no apparent loss of function, but the animal appears a trifle more rash. It can balance, swim, play, feed, distinguish between a worm and a piece of string, and select red wafers from an assortment of various colors thrown upon the water.[4]

We have some experiments upon the shark. If the brain is cut off in front of the thalamus, it can not feed, but retires to one side of the aquarium. The animal can see, but this is of no value to it. The same effect was produced when the olfactory lobes alone were cut off. The shark depends upon its sense of smell, and, since cutting of the cerebral hemispheres, deprived it of its most important sense, the effect, as we have seen, was serious. The relation of the dominant sense to the cerebral hemispheres must always be borne in mind in estimating the value of these experiments. For when the relation of this sense to the hemispheres is such that it must be necessarily injured, the effect will be far more serious than in those cases where it escapes injury. Witness the removal of the cerebral hemispheres in the shark, where the dominant sense—the olfactory—is necessarily injured, and the bony fish, in which the dominant sense—visual—is not disturbed.

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[1] Zur Physiologie des Gehirnes, 1885.

[2] Pflüger's Archiv., Bd.. 44.

[3] Schrader, Pflüger's Archiv., Bd. 41.

[4] Steiner. Functionen des Central nerven systems und ihre Phylogenese. Zwei e Abtheilung, 1888.

The plasticity of the nervous system is shown by the following experiment:[1] If the cerebrum of a shark be cut out unsymmetrically, forced movements occur; the animal swims in a circle. If a shark be beheaded, the trunk swims in a straight line. If now an animal be taken and an unsymmetrical operation on the brain be performed, so as to obtain forced movements and the animal thereupon be beheaded, the trunk exhibits forced movements — like those before beheading — and which appear independent of any permanent contracture in the muscles. If, however, the time between the production of forced movement and the final beheading be less than eight or ten hours, forced movements in the trunk alone do not occur. It thus appears that a certain length of time is necessary to educate the spinal cord to perform this motion.

LECTURE VI.

Gentlemen: To-night I wish to give some account of the principal explanations which have been offered for phenomena of cerebral localization, and to indicate some of the points of contact between these phenomena and psychology. The explanations that have been offered have been mainly from physiological point of view, and have often lacked a good anatomical foundation. The phenomena which the various authors try to explain may be summarized as follows: The meaning of the movements that follows stimulation of certain portions of the cortex; the meaning of the loss of movement and sensation which follows removal of portions of the cortex; the significance of the permanent or transient character of those symptoms, and where transient, the interpretation of the gradual return of function. Further the degenerations following removal of portions of the cortex are to be explained. The explanations that have been offered have been strongly influenced by the bias of there authors, and differ from one another mainly in the emphasis which they put on similar facts. For example Schiff — Pflueger. XXX, 1883,—was strongly influenced by two previous conclusions, first, that nervous centres were not artificially excitable, and second, that dorsal columns of the spinal cord were the afferent paths for tactile impulses and lesion of them would cause an ataxic disturbance of locomotion. For him the true centres for sensation and motion were in the basal ganglia. The nerve fibers connecting these centres with the spinal cord formed an arch, the summit of which lay more or less close to the surface of the hemispheres. Stimulation of the cortex excited these sensory or aesthesodic fibers and brought about a reflex motion. Deep removal of the cortex injured the efferent or kinesodic fibers and gave rise to secondary descending degeneration. Return of function after injury was due to the taking up of the lost functions by those portions which remained intact. Schiff therefore emphasized the sensory side in his explanation. He looks upon Munk (Functionen der Grosshirnrinde p. 42) as the man nearest the truth in his explanation. Munk was struck by the fact that even out side of the so-called motor regions, stimulation of the

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[1] Steiner, op. cit.

cortex gave rise to movements. These extra motor movements were particularly associated with the organs of special sense and were obtained from regions of the cortex which were later found to be the centres for these special senses. Since then each special sense appears to have definite movements associated with it, Munk was led to regard the motor region of the authors as a cortical centre for tactile sensation, in the wide sense of that term, and the motor responses here were to be compared with the motions of the eye upon stimulation of the visual, or of the ear or stimulation of the auditory region. In order to understand his theory of the restitution of function, we may regard his idea of the visual area, where he has worked out his views in detail. In the first place there is a detailed representation of the retina in the cortex. In the centre of this region are the cells which receive the simple sense impressions and around about this central portion are cells which store visual memories. Removal of the former causes absolute blindness, removal of the latter mind blindness or loss of visual memories. When restitution of function takes place it is by the education of the unoccupied cells in the surrounding regions. If all cells capable of this further development are removed, the animal becomes completely and permanently blind. Wundt has criticised the psychological side of this view with all needful severity and we shall see that the experiments of Goltz satisfactorily do away with any such theory of restitution. Goltz[1] came to the question fresh from the study of the spinal cord and apparently convinced of the general truth of Flourens' view, that there was no specialization of function in the hemispheres. In his general view of localization, Goltz is as far as any one from that which is demanded by the clinical medicine of to-day. At the same time he has contributed a large amount of experimental material which forms one of the most valuable chapters on this subject. In the first place he distinguished sharply between permanent and transient symptoms; the latter were brought about by inhibitions due to the secondary degenerations and other disturbances immediately following the operations. For him what is lost is permanently lost, so that restitution of function is never quite complete; but since each hemisphere is connected with both halves of the body, there may be an apparent return of function due to this fact. When both hemispheres are removed, he is forced to the segmental centres, as explained in the last lecture. In this controversy and especially against Goltz, it has been from time to time urged that the removal of a centre or a region was not complete and hence the functions were not abolished. Here and there some evidence appears that the physiological value of a small portion of the cortex may be out of all proportion to its actual size; but we can say nothing more on this point until it has been subjected to a direct experimental test. Hitzig[2] from the experimental side and Nothnagel[3] have emphasized the idea that disturbances of the muscle sense are the cause of the motor disturbance observed. To this view Bastian[4] has added the suggestion that the motor regions, besides being centres for the muscle sense, were also centres for obscure sensations—kinæsthesis—which informed us of the state of contraction of all the muscles of the body, and thus profoundly influenced the contraction of any given muscle. Bain and Wundt have added the sense of effort as a function of the efferent nerves; but this needs hardly to be taken into account, since Prof. James' criticism of their view. I mention these points to show how much vested interest there is in the various theories of the muscle sense, rather than to emphasize their importance for our present consideration. Brown-Sequard[1] has emphasized the idea of inhibition. For him both sides of the body are represented in each hemisphere of the brain and the usual symptoms which are taken to indicate motor and sensory centres in the cortex, are but the results of the inhibitory effect of lesions acting upon centres elsewhere situated. He attempts to support his view by clinical evidence to show that lesions almost anywhere in the cerebrum may produce similar symptoms. The method of proof is open to grave objections, and Brown-Sequard has few if any disciples. At the same time it is not impossible that some of the instances showing defect on the same side as the lesion may be true and explicable on the assumption of anomilies in the structure of the nervous system.

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[1] Verrichtungen des Grosshirns, 1881.
[2] Untersuchungen 1874.
[3] Virchow's Archiv. 1873.
[4] Brain as an organ of mind, 1880.

Without further entering upon the history of these views, I will at once proceed to give the explanation so far as it seems well supported by anatomy. Goltz has shown that the disturbances in locomotion and other movements cannot be properly explained by referring them to defects in sensation. For our general explanation we come back to the idea developed in the last lecture of a short segmental, or a long central path. To use the term sensory with regard to the afferent and motor with regard to the efferent portion of the central path is to a certain extent misleading. The terms are, however, in common use, and, if we can escape attaching too much value to them, are satisfactory. The course of the incoming impulse over the sensory path is yet to be made out. On leaving the cortex by the motor path its course seems comparatively clear. It is quite impossible to say whether in stimulating the cortex we stimulate the sensory cortical elements, and thus influence the motor ones or stimulate the motor directly. Any general scheme must also explain the restitution of function and this term may stand for a number of different events, especially in the higher animals. As Horsley[2] has shown the restitution of function in the case of hemorrhage into the internal capsule may be well associated with the resorption of the clot. Again the muscles of the phonation and mastication have in man a bilateral cortical representation so that as a rule these muscles on both sides of the body are represented in each hemisphere. Here of course the explanation of restitution is comparatively simple and it is this explanation which is also used in case of dogs with a single hemisphere. In fact bilateral representation appears to increase as we pass down the vertebrate series. There seems to be no evidence to show that when the arm is paralyzed through cortical lesion that there is here, in man at least, a restitution of function. But there is still another sort of restitution which differs mainly perhaps from the fact that it is more complex. I refer to such cases as those in which articulate speech is reacquired after destruction of the motor speech centre upon one side. About such an instance cluster a number of interesting problems. We know that as a rule for speech, both motor and sensory, the left hemisphere is the more important. Why this is the case is nevertheless not clear. That the important sensory and motor centres, which are in practice interrelated, should come to be in the same hemisphere, seems a natural result of the better anatomical connections between these centres on the same side of the brain; but whether in the determination of this side the motor or sensory element takes the lead, we cannot say. At the same time it would appear that both hemispheres of the brain share in the education even in those cases where the exercise seems to be limited to one side of the body. If this is so, then, the reacquisition of language by an adult, after loss of the motor centre for speech on one side, would perhaps be dependent upon this double education of the brain and the possibility of establishing connections between the sensory and motor centres in the hemisphere which had not been explicitly exercised. This necessarily brings to our consideration the manner in which we are to think of these associations as taking place. The long period of helplessness in the highest mammalia, the evident effect of training and exercise in the earliest years of life, would seem at first to point to the establishment of new morphological relations as the result of functional activity. A closer examination brings out a good deal of evidence against this view. If functional superiority has a morphological basis, then the left side of the brain should in most instances be the more largely developed. In the average individual the difference in weight between the two hemispheres lies within the errors of observation. We must consider that, if a morphological change is brought about, it is practically the same in both hemispheres. Further, in Laura Bridgman, the portions of the hemispheres connected with her defective senses, though in some instances slightly abnormal, were by no means lacking nor histologically degenerate. So far as we know she had neither visual or sensory memories. Considering growth and function as closely related, then the growth in these portions of her hemispheres was certainly remarkable. I am inclined to the view that the morphological characteristics of a brain are very early fixed and that education has to do mainly with functional developments hence, in the case we have been considering, the reacquisition of speech would depend on association paths which had already existed. The sensory regions of the cortex have a peculiar interest and value, for it seems on the one hand that the ideational of processes are most closely linked with the sensory regions and that on the other hand a single sensory region may serve as the basis for an intellectual life: witness the mental development of the blind deaf-mutes. It may be safely said that in acuteness, man is surpassed by some animal in the case of every sense. Man is peculiar in the high development of several senses and in the ability for cross-reference between them, so that, although each principal sense at least would seem to be sufficient for a basis of an intellectual existence, and thus each sensory region might be considered a little brain, yet fullness of intellectual development would appear to associate itself with a high simultaneous development. On this point the manifold symptoms of aphasia are most instructive.

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[1] Lancet, 1876, 1877.

[2] Lancet, 1884.

NOTES ON MODELS OF THE BRAIN.

Henry H. Donaldson, Clark University.

1. The use of models of the brain as one means of instruction requires no apology. In view of the interest which at present attaches to such models, I have made a list of the principal ones with some annotations. At my suggestion, Mr. T. L. Bolton has prepared a translation of the description of the large brain-model manufactured by Auzoux. This model appears to be, on the whole, the most instructive one, and, as the original description was in French, the anatomical terms of which are, as a rule, unfamiliar to our students, it was thought that such a translation of the description would make it more generally useful.

List of Models.

1. Aeby's wire model of brain and cord: (Phantom des Faserverlaufes im menschlichen Gehirn und Rückenmark von Prof. Dr. Chs. Aeby). Made by F. R. Büchi, Meckaniker, Berne, Switzerland. Price, 500 francs. Material, wire and cork. Shows the path of the fibres according to Wernicke and is enlarged about six diameters. Useful from the fact that, though giving the relations in three dimensions, it is transparent.

2. Auzoux (Mme. Ve Auzoux, 56 Rue de Vanguard, Paris). The synthetic preparation of the brain (Cerveau de Texture de tres-grande

dimension. Prix, 300 Fr. Enlargement about two diameters.) is the one to which the following description applies. It is a valuable preparation from the fact that is shows the inter-relations of the deep portions. Auzoux makes a number of other preparations of both brain and cord, which are less elaborate and for which the reader is referred to his catalog. Material: Papier maché.

3. Exner. In the physiological institute of the University of Vienna, there is made a plaster cast of cerebrum—life size—on which are marked the cortical centres as determined by Exner,-vide. "Localisation der Functionen in der Grosshirnrinde des Menschen." Price, 10 Marks.

4. Fick. Phantom des Menschenhirns. Ludwig Fick—natural size—paper. Price, 3 M. This is an attempt to show the relations by layers of paper cut in the proper shape and colored. It is a very helpful device.

5. Tramond. Maison Vasseur, 9 Rue de l'école de medicine, Paris. Among the very numerous models made by this firm, a number relate to the central nervous system. "Anatomie du bulbe rachidien grossi par 13 coupes, d'après Mathias Duval." Price, 80 francs. (Plaster of Paris.) The transverse sections admit of identifying the appearance of a section with its level in the medulla. The preparation is rather rough, especially in the painting of the sections.

6. *Ziegler.* Dr. Adolph Ziegler in Freiburg. Baden, Germany. This maker works in wax, and his preparations are both accurate and artistic. The gyri of human brain — Ecker — shown in two pieces — life size. 30 Marks. The development of the gyri in the human brain—Ecker—shown in 14 pieces—life size. 54 Marks. Form of brain in the vertebrate series: 8 pieces, somewhat enlarged. Price, 136 marks. These three sets of models are very useful for the study of the development of the gyri in man and for a general idea of the comparative development of the brain in vertebrates.

The above list represents but a few of the available models, but I believe it contains those which will be found most serviceable for ordinary purposes. A model can be constructed by enlarging cross sections of a part and drawing them on large panes of glass. These panes arranged in series in a frame thus give a fair idea of the part, being at the same time transparent. We have thus constructed a model of the spinal cord in man. An accurate opaque model can be made from plates of wax of proper thickness, on which the enlarged sections of an object are outlined, and then the portion so outlined is cut out. These plates of wax, when piled together, will then represent the object enlarged so many diameters.

To the following translation I wish to preface a few words. The model described is based on the views of Dr. Luys. In some instances these can be shown to be wrong, and, in others, open to serious objections. In the main they agree with the anatomy of the day. It is not our purpose to edit the description, any further than is needful for the present case. All that appeared superfluous in the original description has been omitted in the translation. After the erroneous or doubtful designations, we have for the most part contented ourselves with putting [Luys] in brackets, to designate that the view presented by that author was not generally accepted.

We have introduced some synonyms. In the cases which are most complete, the English, Latin, Terms of Wilder, French and German are given in the order named, but we have been guided here more by what we thought would be of assistance, than by the idea of formal completeness.

BRAIN MODEL ON A LARGE SCALE.

By Dr. Auzoux.

Translated by T. L. Bolton.

Explanatory Remarks.—An ordinal number preceded by the sign ☞, indicates that the piece upon which it is placed may be detached; the small numbers or letters of the alphabet indicate the details.

In this new edition of the human brain, the course of the nerve-fibres can be traced through all the parts of the encephalic mass. This preparation was constructed from dissections made on normal brains hardened in chromic acid according to the directions of Dr. Luys.[1] . . . It enables one to see many details in the cerebrum, in the cerebellum, in the pons, in the medulla oblongata, and in the cephalic part of the spinal cord.

☞ No. 1.

Left half of the callosum — great commissure, corpus callosum, callosum, corps calleux, der Balken.

The callosum, from which the cerebral mass is almost completely separated, represents a kind of box which forms the walls of the lateral ventricle.

The three kinds of nerve-fibres which enter into the composition of the cerebrum are represented in arbitrary colors of different shades. They are designated by the names: Afferent or sensory fibres, efferent or motor fibres, and commissural fibres; those which connect the two hemispheres.

1. Dorsal surface of the callosum.

2. Cephalic extremity of the collosum — the knee, genu corporis callosi, genu, genou die Balkenknie.

3. Reflected portion of the callosum—*E*—, rostrum corporis callosi, rostrum, bec, der Schnabel.

4. Caudal extremity of the callosum corresponding to the splenium—*E*—, splenium corporis callosi, splenlum, bourrelet, der Balkenwulst.

5, 5. Longitudinal tracts — nerves of Lancisi — mesal longitudinal striæ, striæ longitudinales mediales, *W*—, nerfs de Lancisi, *G*—.

6, 6, 6. Transverse tracts — transverse striæ, *L*—, *W*—, tractus transvesaux, *G*—.

7 *a*, *a*, *a*. Afferent or sensory fibres [fibres convergentes supérieures (Luys)].

8 *b*, *b*, *b*. Commissural fibres [fibres commissurantes (Luys)].

9 *c*, *c*, *c*. Efferent or motor fibres [cortico-striées (Luys)].

10. Portions of the corona of Reil — fibrous cone, corona radiata corona, couronne de Reil, der Stabkranz.

11. Ventral surface of the callosum.

12. Sphenoidal boundary of the lateral ventricle (étui de l'hippocampe).

13. Posterior horns of the lateral ventricle — posterior horn, cornu posterius, postcornu, cavité digitale, das Hinterhorn.

14. Unciform eminence, calcar avis, calcar, ergot de Morand, der Vogelsporn.

15. Calcarine fissure, fissura calcarina, calcarine fissure, repli de la circonvolution de l'ergot, *G*—.

☞ No. 2.

Left half of the fornix,—*E*—, fornix, fornix, voûte à trois piliers, die Gewoelbe.

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[1] Recherches sur le système nerveux cérébro-spinal,—sa structure, ses fonctions et ses maladies,—accompagné d'un atlas, par J.-J. Luys, Paris, 1865.

Upon this portion may be noticed the anterior pillar of the fornix, its continuation with the hippocampus major, the insertion of the afferent fibres of this region in the gyri of the hippocampus, the commissural fibres that arise in the cortical cells and unite to form the lyra, and how as fibres of the lyra, on reaching the anterior pillar they decussate to form the anterior commissure (Luys).

1. Anterior pillar, columna fornicis anterior, fornicolumna, pilier antérieur, vorderer Gewoelbeschenkel.

2. Postérior pillar, columna fornicis posterior, *W*—, pilier postérieur, hinterer Gewoelbeschenkel.

3. A portion of the lyra—*E*—, lyra, lyra, lyre, die Leier.

4. *E*—, hippocampus major, hippocampus, hippocampe ou corne d'Ammon; Ammonshorn.

5. *E*—, fimbria, fimbria, bandelette de l'hippocampe ou corps bordant, der Saum.

6. Hippocampal gyrus—*E*—, gyrus hippocampus, *W*—, circonvolution de l'hippocampe, *G*—.

7. *E*—, uncus, uncus, crochet, der Haken.

8. The free border of this gyrus which is continued upon the callosum under the name of the nerve of Lancisi, is called dentate convolution.—fascia dentata, fasciola, corps godronné, gezähnte Leiste.

9. The cortical or gray matter—ectocinerea—composed of two layers of cells.

10. The white or fibrous matter—alba (*W*).

11. A transverse section of the hippocampus showing the windings of the medullary fibres and their insertion in the cortical cells.

12. Termination of the anterior commissure—commissura anterior, præcommissura, commissure antérieure, vordere Commissur—in the cephalic portion of the sphenoidal lobe.

13. Posterior extremity of the callosum—the splenium of the authors.

☞ No. 3.

Superior portion of the left optic thalamus—thalamus opticus, thalamus, couche optique, der Sehhügel.

This section shows a portion of the three posterior centres of the thalamus, the fibres of the optic nerve passing from the geniculate bodies to the mesal centre.

1. The thalamus showing:

2. The mesal centre receiving the optic fibres—inner nucleus, nucleus cinereus internus, *W*—, centre moyen (Luys), innerer Kern.

3. The median centre receiving the fibres of the dorsal column of the spinal cord [centre médian (Luys).] (Considered by other authors as a portion of the lateral nucleus.)

4. Posterior centre—centre postérieur—receiving the auditory fibres.

5. Plexiform disposition of the fibres that pass from the thalamus to enter into the formation of the corona.

6. The central tubular gray matter—*L*—, entocinerea, substance grise centrale, centrales Höhlengrau—which covers over the thalamus.

7. Optic tract, tractus opticus, tractus opticus, bandelette optique, der Sehstreif.

8. External root passing to the external geniculate body—corpus geniculatum externum, geniculatum externum, corps genouillé externe, äusserer Kniehöcker.

9. Internal root passing to the internal geniculate body—corpus geniculatum internum, geniculatum internum, corps genouillé interne, innerer Kniehöcker.

10. External geniculate body.

11. Internal geniculate body.

12. Fibres passing from the geniculate bodies to the corpora quadrigemina—quadrigeminal bodies, corpora or tubercula quadrigemina, corpora quadrigemina, tubercules quadrijumeaux, die Vierhügel.

13. Optic fibres passing from the geniculate bodies to the mesal centre of the thalamus; the fibres are laid bare by the removal of the central tubular gray matter.

☞ No. 4.

Upper portion of the intra-ventricular corpus striatum — caudate nucleus, nucleus caudatus, striatum caudatum, corps strié intra-ventriculaire, geschwänzter Kern.

☞ No. 5.

Annular protuberance or Pons Varolii—tuber annulare, pons, pont de Varole, die Brücke—

Showing the decussation of the fibres of the middle peduncle of the cerebellum with the longitudinal fibres of the ventral peduncles of the cerebrum — a decussation which gives to this portion the appearance of a mat.

1. Fibres of the middle peduncle of the cerebellum—*E*—,crus ad portem, medipedunculus, pédoncle cérébelleux moyen, der Brückenschenkel.

2. Decussation of these fibres in the median line.

3. The decussation of these same fibres with the fasciculus of the ventral pyramids — pyramides anteriores, pyramides, pyramides antérieures, die Pyramiden.

4. The fibres of the pyramids.

5. Trigeminal nerve or fifth pair composed of two fasciculi—nervus trigeminus, *W*—, nerf trijumeau, die dreigetheilte Nerv.

6. Motor fasciculus of the same nerve.

7. Sensory fasciculus dividing into two branches.

8. A branch uniting in the formation of the fillet — lemniscus, lemniscus, fillet de Reil, die Schleife.

9. A branch passing towards the central gray matter of the axis.

10. Abducent nerve or sixth pair — nervus abducens, *W*—, nerf moteur oculaire externe, aüsserer Augenmuskelnerv.

☞ No. 6.

Right half of the cerebellum — hind-brain, cerebellum, cerebellum, cervelet, das Kleinhirn.

1. Exterior surface of the cerebellum.

2. Median lobe or vermis of the cerebellum—the worm or vermiform process, lobus cerebelli medius, vermis, vermis du cervelet, der Worm.

3. Lateral lobe of the cerebellum.

4. Lobules or subdivisions of the cerebellar hemispheres.

5. Superior peduncle of the cerebellum—crus ad cerebrum, præpedunculus, pédoncle cerebelleux supérieur, der Bindearm.

6. Middle peduncle of the cerebellum.

7. Inferior peduncle of the cerebellum — crus ad medullam, postpedunculus, pédoncle cérébélleux inférieur, der Medullarschenkel.

8. Valve of Vieussens, velum medulare anterius, valvula, valvule de Vieussens, vorderes Marksegel.

9. Disposition of the fibres of the cerebellum — *E*—, arbor vitæ, arbor, arbre de vie, der Lebensbaum.

10. The folds of the cortical matter forming the folia.

11. Section through the lateral lobe.

12. Rhomboidal or dentate body, corpus dentatum, dentatum, corps dentalé, *G*—.

13. The fibrous portion.

14. Cellular portion.

15. Divergence of the white fibres.

16. Nuclei of the gray matter lying among the foregoing fibres. [Luys]
17. Section of the cortical gray matter.
18. *E*—, uvula, cerebelli uvula, luette, die Zapfe.
19. Valve of Tarini, or posterior medullary velum, velum medulare posterius, metatela, valvule de Tarin, hintere Hirnklappe.
20. Lobule of the pneumo-gastric—*E*—, lobulus appendicularis, flocculus, lobules du cervelet, die Flocke.

☞ No. 7.

Lemniscus or ribbon of Reil and the left half of the corpora quadrigemina.

The fibres which compose the lemniscus have three distinct origins, and after having decussated on the median line, come to lie dorsad of the Iter, and are distributed to the posterior and median centres of the thalamus. The dorsal fibres of this tract decussate and form the posterior commissure [Luys].

1. Anterior tubercle, corpus quadrigeminum anterius, prægeminum, tubercule supérieur, obere Zweihügel.
2. Posterior tubercles, corpus quadrigeminum posterius, postgeminum, tubercule inférieur, untere Zweihügel.
3. Central tubular gray matter covering the fourth ventricle.
4. Fasciculus formed by the trigeminal nerve (Luys).
5. Fasciculus formed by the auditory nerve—nervus auditorius, *W.*—, nerf acoustique, der Hörnerv.
6. Fasciculus formed by the spinal cord—medulla spinalis, myelon, moelle épinière, das Rückemmark.
7. Decussating fibres.
8. Portion of the aquæduct Sylvii, aquæductus, l'aqueduc de Sylvius, die Wasserleitung.
9. Portion of the valve of the Vieussens.
10. Trochlear nerve or fourth pair—nervus trochlearis, *W.*—, nerf pathétique, der Rollmuskelnerv.

☞ No. 8.

Left ventral column of the spinal cord, ventral peduncles of the cerebrum (the ventral pyramids of the authors.)

This portion is designed to show how all the efferent fibres (cortico-striées-Luys) traverse the extra-ventricular portion of the corpus striatum, thus forming the three arches 3, 4, 5; and how, after having formed the three arches, the cortical striæ form the three cones which constitute the ventral peduncles of the cerebrum, that is, the motor tracts.

1. Extra-ventricular nucleus of the corpus striatum cut vertically—lenticular nucleus, necleus lenticularis, lenticula, noyau extra-ventriculaire du corps strié, der Linsenkern.
2. The termination of the fibres of the superior peduncle of the cerebellum in the three arches of the corpus striatum. (Luys.)
3. Internal arch.
4. Middle arch.
5. External arch.
6. Locus niger of Vicq-d'Azyr. (Substantia nigra of the authors.)
7. Tuber cinereum, torus, corps cendré, *G.*—
8. Infundibulum, infundibulum, infundibulum, tige pituitaire, der Trichter.
9. Corps cendré=tuber cinereum.
10. Optic tract.
11. Optic commissure—chiasma, chiasma nervorum opticorum, chiasma, chiasma des nerfs optiques, die Sehnervenkreuzung.
12. (Omitted.)
13. Olfactory tract, tractus olfactorius, *W*—, nerf olfactif, *G.*—

14. Mesal root. (Luys.)
15. Median root. (Luys.)
16. Median root of the right side passing to the olfactory ganglion of the left side. (Luys.)
17. The fibres that bind the olfactory ganglion to the central gray matter. (Luys.)
18. Lateral root, stria externa, *W*—, racine externe, laterale Wurzel.
19. Olfactory ganglion. [Luys]
20. Portion of the tænia semi-circularis or stria cornea, tænia, lame cornée, der Grenzstreif.
21. Anterior commissure—commissura anterior, præcommissura, commissure antérieur, vordere commissur—exposed by the removal of a portion of the corpus striatum.
22. Oculo-motor nerve, or third pair, nervus oculomotorius, *W*—, nerf moteur oculaire commun, der Augenmuskelnerv.
23. Trochlear nerve or fourth pair.
24. Anterior peduncles of the cerebrum formed by the three cones.
25. (Omitted.)
26. (Omitted.)
27. (Omitted.)
28. The fibres of the middle peduncles of the cerebellum intercrossing with the fibres of the cerebral peduncles.
29. The fibres forming the sensory root of the trigeminal nerve, or the fifth pair.
30. The fibres forming the motor root of the fifth pair.
31. The fibres forming the root of the abducent nerve or sixth pair.
32. After-brain—medulla oblongata, oblongata, bulbe, das verlängerte Mark—upon which may be distinguished:
33. Anterior pyramids, corpus seu eminentia pyramidalis, pyramis ventralis, pyramide antérieure, *G*—;
34. Left olivary body—oliva, oliva, olive, die Olive—cut to show the arrangement of the arcuate fibres—fibræ arciformes, *W*—, fibres arciformes, die Bogenfasern.
35. Arcuate fibres derived from the inferior peduncles (Luys) of the cerebullum decussating in the middle line.
36. The hypoglossal nerve or twelfth pair—nervus hypoglossus, *W*—, nerf grand hypoglosse, der Zungenfleischnerv.
37. Ventral column of the spinal cord.

☞ No. 9.

Dorsal peduncle of the cerebrum—tegmentum, tegmentum, étage supérieur du pedoncle, das Haubenfeld.

This portion is designed to show how the nerves which transmit to the cerebrum the sensations from all parts of the body are focused in the four centres of the thalamus, and how the fibres of the thalamus, changing their direction, pass below the tænia semi-circularis to unite in forming the corona.

1. Portion of the corpus striatum—nucleus caudatus.
2. Portion of the corpus striatum—nucleus lenticularis.
3. The thalamus cut transversely.
4. Cephalic or olfactory centre—tuberculum anterius, *W*—, centre antérieur, die vorderer Kern—of the thalamus.
5. Mesal or optic centre.
6. Median or inferior centre.
7. Posterior or acoustic centre—tuberculum posterius, pulvinar, centre, postérieur, der Polster.
8. Posterior commissure, commissura posterior, postcommissura, commissure postérieure, hinterer Commissur.
9. Gray or soft commissure of the thalamus—middle commissure, commissura mollis, medicommissura, commissure grise, *G*—.

10. Mammillary bodies, corpora mammillaria, albicans, éminence mammillaire, die Markkügelchen.

11. Foramen of Monro, foramen Monroi, porta, trou de Monro, *G*—.

12. Anterior pillar of the fornix.

13. Anterior fibres of this pillar proceeding to the gray matter of the septum lucidum—hemi-septum, *F*—, die Scheidewand—and to the corpus striatum.

14. Posterior fibres forming the peduncle of the pineal gland—pedunculus cornarii, habena, pedoncle de la glande pinéale, *G*—.

15. The fibres of this peduncle communicating with the anterior centre of the thalamus.

16. Descending fibres proceeding to the corpora mammillaria.

17. Bundle of Vicq-d'Azyr—radix descendens fornicis, *W*—, fascicule de Vicq-d'Azyr *G*—, proceeding from the anterior or olfactory centre to the corpora mammillaria.

18. Tænia semicircularis.

19. Superior olivary body or the corpus of Stilling—tegmental nucleus, nucleus tegmenti (erroneously called *olive supérieure* by Luys), der rothe Kern.

20. The depression in this body designated as the hilum.

21. Left superior peduncle of the cerebellum.

22. The fibres of this peduncle decussating in the middle line.

23. Inferior peduncle of the cerebellum.

24. The fibres of this peduncle intermingling with those of the dorsal column of the spinal cord.

25. The dorsal column—columna posterior, *W*—, cordon postérieur, *G*—, —of the spinal cord.

26. Restiform body,[1]—corpus restiforme, restis, corps restiforme, das strangförmige Körper.

27. Posterior pyramid,[2] clava funiculi gracilis, clava, pyramide postérieure, *G*—.

28. Floor of the fourth ventricle.

29. Calamus scriptorius continuous with the central canal of the spinal cord.

30. Iter.

31. Gray matter of the axis passing across which the ascending fibres of the posterior peduncle of the cerebellum may be seen.

32. Dorsal column of the spinal cord.

33. Entrance of these fibres in the median centre of the thalamus. [Luys.]

34. Plexiform arrangement of the fibres leaving the centres of the thalamus.

35. Their course below the tænia semicircularis.

36. These fibres ascending to form the corona.

37. Corona.

38. The root of the oculo-motor nerve or the third pair.

39. The root of the trochlearis or fourth pair.

40. Origin of the trigeminal nerve or fifth pair.

41. The motor root of the trigeminal nerve.

42. The root of the abducent nerve or sixth pair.

43. The facial nerve or seventh pair,—nervus facialis, *W*—, nerf facial, der Gesichtsnerv.

44. Nerve of Wrisberg, nervus intermedius, *W*—, nerf de Wrisberg, *G*—.

45. Acoustic nerve or eighth pair.

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[1] The number and designation should include the more lateral portions of the dorsal column.

[2] The designating number 27 is placed upon the funiculus cuneatus instead of upon the funiculus gracilis, where it should stand.

46. The ganglionic enlargement of this nerve—tuberculum acusticum.
47. Root of this nerve losing itself in the central gray matter of the fourth ventricle (striæ medullares or striæ acusticæ of the authors.)
48. The fasciculus of the acoustic nerve contributing to the formation of the leminiscus.
49. The glosso-pharyngeal nerve or ninth pair—nervus glosso-pharyngeus. *W*—, nerf glosso-pharyngien, der Zungenschlundkopfnerv.
50. The pneumogastric nerve or tenth pair—vagus nerve, nervus vagus, nerf pneumogastric, der Herumschweifendenerv.
51. The spinal accessory nerf or eleventh pair—nervus accessorius, *W*—, nerf spinal *G*—.
52. The tubercle of Rolando—tuberculum cinereum Rolandi, *W*—, substance gelatineuse de Rolando, *G*—.
53. The ventral horn of the gray matter of the spinal cord.
54. The dorsal horn of the gray matter of the spinal cord.
55. The ventral or motor root of the spinal accessory nerve.
56. The dorsal or sensory root of the spinal accessory nerve.
57. The root of the hypoglossal nerve or twelfth pair.
58. First pair of cervical nerves.
59. The ventral or motor root of this nerve.
60. The dorsal or sensory root of this nerve.
61. Accessory nerve [Vaso-motor root of the great sympathetic nerve. Luys].
62. Spinal ganglion.

☞ No. 10.

Part of the cortical gray matter of the right hemisphere, on which the gyri and sulci are seen.

1. Fissure of Sylvius.
2. Fissure of Rolando—central fissure, fissura centralis, central fissure, scissure de Rolando, *G*—.
3. The gyri of the island of Reil—insula.
4. The external cortical layer composed of small cells.
5. Internal cortical layer composed of large cells.

☞ No. 11.

Right cerebral hemisphere.

This portion is designed to show in their totality all the parts of the cerebrum and to give an idea of their functions.

1, 2, 3. First, second and third frontal gyri.
4. The fissure of Rolando.
5, 5, 5, 5. Superior or parietal gyri.
6, 6, 6. Posterior or occipital gyri.
7, 7. The fissure of Sylvius.
8. Temporo-sphenoidal lobe.
9. The gyri of the island.
10, 10, 10. The gyri of the mesal surface of the hemisphere.
11. Gyrus fornicatus.
12. Hippocampal gyrus.
13. Fascia dentata.
14. The uncus.
15. The callosum.
16. The splenium of the callosum,
17. The genu of the callosum.
18. The rostrum of the callosum.
19. Longitudinal tract—the nerves of Lancisi.
20. The fornix.
21. The anterior pillar.
22. The septum lucidum composed of two laminæ of which one has been in part removed.

23. The lyra.
24. The thalamus on which four centres are seen.
25. The anterior or olfactory centre.
26. The mesal or optic centre.
27. The posterior or acoustic centre.
28. The median or inferior centre.
29. The pineal gland, glandula pinealis, conarium, glande pinéale, die Zirbeldrüse.
30. The peduncles (habenulæ of the authors.) of the pineal gland separated into two fasciculi.
31. Fasciculus going to the anterior centre of the thalamus.
32. Fasciculus going to the anterior pillar of the fornix. (Luys.)
33. Corpus mammillare.
34. Bundle of Vicq-d'Azyr passing from the anterior centre of the thalamus to the corpus mammillare.
35. The third ventricle.
36. The foramen of Monro.
37. The aqueduct of Sylvius.
38. Anterior commissure.
39. The termination of the anterior commissure in the temporo-sphenoidal lobe.
40. The gray or soft commissure.
41. The posterior commissure composed of three distinct fasciculi intercrossing with those from the opposite side.
42. The tuber cinereum.
43. Infundibulum.
44. Anterior portion of the lateral ventricle.
45. Corpus striatum—nucleus caudatus.
46. Corpus striatum—nucleus lenticularis—separated from the preceding to show the insertion of the cortico-striæ.

47, 47. The termination of the superior peduncles of the cerebellum in the arches of the corpus striatum. [Luys.]

48. The temporo-sphenoidal portion of the lateral ventricles.
49. The hippocampus or Ammonshorn.
50. The fimbria.
51. The fascia dentata.
52. The occipital portion of the lateral ventricles, or the digital cavity.
53. The hippocampus minor.
54. The corpora quadrigemina.
55. The corpus prægeminum.
56. The corpus postgeminum.
57. The great transverse fissure (fente de Bichat)—the space included between the splenium of the callosum, the corpora quadrigemina, and the gyri fornicati. Within this space are to be seen:
58. The geniculate bodies;
59. The fasciculi joining these bodies with the corpora quadrigemina;
60. The floor of the fourth ventricle.
61. The calamus scriptorius.
62. (Omitted.)
63. The superior peduncle of the cerebellum.
64. The decussation of this peduncle with that of the opposite side.
65. The tegmental nucleus.
66. The inferior peduncles of the cerebellum.
67. Tubercle of Rolando.
68. The restiform bodies.
69. The column of Goll—funiculus gracilis, *W*—, funicule grêle, die zarte Stränge.
70. The nucleus of the funiculus gracilis—clava.
71. The olfactory bulb—bulbus olfactorius, rhinobulbus, bulbe olfactif, der Riechkolben.

72. The olfactory tract.
73. The mesal root of this tract.
74. The middle or gray root.
75. The external root going to the ganglion.
76. The olfactory ganglion.
77. The mesal or gray root of the left olfactory tract proceeding to the olfactory ganglion of the right side.
78. The fibres binding the olfactory ganglion to the central gray matter.
79. Tænia semicircularis.
80. The optic nerve.
81. The chiasma.
82. The fasciculus of gray fibres connecting the chiasma of the optic nerves with the tuber cinereum. (Erased in original.)
83. The tract of the optic nerve dividing into two fasciculi.
84. The fasciculus passing to the internal geniculate body.
85. The fasciculus passing to the external geniculate body.
86. Perforated space—anterior perforated space, locus perforatus anticus, præcribrum, quadrilatère perforé, *G*—, —limited posteriorly by the optic fibres, laterally by the fascia dentata, and anteriorly by the roots of the olfactory nerves.
87, 87, 87. The anterior cerebral peduncles, on which are seen the three layers coming from the lenticular nucleus.
88. The root of the common oculo-motor nerve or third pair.
89. The trochlear nerve or fourth pair.
90. The sensory root of the trigeminal nerve or the fifth pair.
91. The motor root of the same nerve.
92. The root of the abducent nerve or the sixth pair.
93. The root of the facial nerve or the seventh pair.
94. Acoustic nerve or eighth pair.
95. Tuberculum acusticum.
96. Lemniscus formed of three roots.
97. Fasciculus of the acoustic nerve [Luys.]
98. Fasciculus coming from the trigeminal nerve (Luys).
99. Fasciculus coming from the spinal cord.
100. Decussation of the lemniscus forming the superior wall of the iter.
101. Part of the velum medullare anterius.
102. The nerve of Wrisberg.
103. The glossopharyngeal nerve or the ninth pair.
104. The pneumogastric nerve or the tenth pair.
105. The spinal accessory nerve or the eleventh pair.
106. The ventral or motor root of the spinal accessory nerve.
107. The dorsal or sensory root of the same nerve. [Luys].
108. The root of the hypoglossal nerve or the twelfth pair.
109. The first pair of cervical nerves.
110. The ventral or motor root of the same pair.
111. The dorsal or sensory root of the same nerve.
112. Spinal ganglion of this same nerve.
113. Vaso-motor fibres of sympathetic nerve. [Luys].
114. Second pair of cervical nerves showing the same character and details as the first pair.
115. Spinal cord.
116. The ventral longitudinal fissure.
117. The dorsal longitudinal fissure.
118. The ventral, anterior or motor column.
119. The dorsal, posterior or sensory column.
120. Lateral column.

121. Gelatinous substance of Rolando in the centre of which is seen the central canal of the spinal cord.

122. The central canal.

123. The ventral horns of the spinal cord in connection with the efferent or motor fibres.

124. The dorsal horns in connection with the afferent or sensory fibres.

125. The central gray matter of the spinal cord.

126. Claustrum, claustrum, avant-mur, vormauer — lying between the lenticulate nucleus and the gyri of the island.

127. Antero-posterior fasciculus composed of afferent fibres, *a*, *a*, *a*, and of efferent fibres, *c*, *c*, *c*, which form the connection between the anterior gyri of the cerebral lobes and the most distant part of the corpus striatum and the thalamus, and which form a kind of enclosure in which the claustrum lies (connections those of Luys).

128. Inter-cortical commissural fibres — association fibres of Meynert.

129. Ventral column of the spinal cord separating from that of the opposite side to allow the lateral columns to decussate.

130. The decussation of the pyramids.

131. The decussation of the dorsal columns.

132. The ventral pyramids.

133. Substantia nigra lying between the two fasciculi which form the anterior pyramids.

134. The ventral layer of the cerebral peduncle — crusta, crusta, *F*—, Hirnschenkelfuss.

135. The superior layer of the cerebral peduncle formed by the dorsal and ventral columns of the spinal cord.

A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

By Edmund C. Sanford, Ph. D.

After Prof. Ladd's careful statement of the psycho-physiological facts and Prof. James's brilliant exposition of their psychological and even metaphysical import, it is no longer necessary to argue the importance of the subject matter of this branch of the new psychology. No one that has once seen the new is going to be satisfied any longer with the old. But the appropriation of new facts alone is not sufficient to elevate psychology to its true place in the circle of sciences. As long as psychologists live upon the crumbs that fall from the tables of neurology and physiology they will live in dependence. They must investigate for themselves,—no less rigorously and no less broad-mindedly than others, but from their own standpoint, and must view what they find in its psychological perspective. This means that a prominent place must be given to psychological laboratories for research; and the friends of psychology already congratulate themselves on the beginning of several of great promise in this country.

Beyond this, however, lies another thing of cardinal importance, namely, the adoption of a right pedagogical method. The student of psychology must have its facts and principles brought home to him in a way not inferior to the best in other sciences, if psychology is to have the infusion of new vigor that they have had, and afford the healthy and virile training that they afford. He must see for himself the phenomena about which he psychologizes, he must perform the experiments, he must have the inside view. The new psychology has been said to do away with introspection, but that is a mistake. It retains introspection and refines and gives it precision by making it operate under experimental conditions; and it is just these inner aspects that are particularly hard for the student to frame for himself from bare descriptions. He must himself serve as subject of the experiment before he can really understand it. To say, as has recently been said, that a few models of the brain and a color-mixer are about all the apparatus needed for a course in physiological psychology savors of the scholasticism from which we hope to have escaped. Notwithstanding its better material, such a method must lead to the same text-book work and the same artificial general conceptions as of old. For those especially that are to work in any of the fields of applied psychology, in pedagogy, or criminology, or even theology, the intimate laboratory knowledge (and its parallels in anthropological and comparative psychology) is essential to an effective grasp of their subjects. The need of such an apprenticeship for later work in the research laboratory is of course obvious. That such a course is even now desired by open eyed teachers is shown by the inquiries made for it of those known to be engaged in experimental psychological work.

Just what experiments such a course should contain is itself as yet a matter of experiment; but that it should, if it aims at any thing like proportion, introduce the student to all the chief methods of research and cause him to observe for himself all the more important phenomena seems reasonable. Such a course has been in mind in the collection of the experiments which is begun below, and which is to be continued in successive numbers of the JOURNAL till completed. That the list is complete or the selection always the best the author is very far from maintaining — to mention a large omission only, no experiments on hypnotism are now proposed, because they seem unfitted to beginners in the field. And in any event the ideal laboratory course can only be reached after repeated adaptation and long trial in actual use. This course had its origin in a series of notes which it was found necessary to make for the use of a group of students taking my practice course during the past year. The experiments have been performed in the laboratory here, and all, except those added in this revision, by the students themselves. The demonstrational character of the work has been kept in mind, and the experiments chosen are generally rather qualitative than quantitative, even where for convenience they have been given a quantitative form. In selecting apparatus the simplest that promised the desired result has generally been chosen; and while this makes the course by no means representative of the facilities of this laboratory, much less of the possibilities of psychological experimentation, it may perhaps make it useful to those teachers—unfortunately too many—whose equipment must be brought within the compass of a scanty appropriation. A large part of the absolutely essential apparatus could be made by the teacher himself, and almost all, I doubt not, with the assistance of common mechanics. The notes on apparatus and references to literature that are inserted from time to time will open the way to more elaborate experiments and apparatus for those that desire them.

I.—THE DERMAL SENSES.

SENSATIONS OF CONTACT.

Apparatus. The experiments on the Sense of Locality require no special apparatus. Those on Discriminative Sensibility can be made with ordinary drawing dividers; but if these are used, it will be well to stick the points into little pointed tips of cork to avoid the sharpness and coldness of the metal. (An excellent, but more expensive, Æsthesiometer is made by C. Verdin, 7 Rue Linne, Paris, at 35 francs; for the description of an elaborate and very convenient one, see AMER. JOUR.

PSYCHOL. I, 552.) Something is also needed in experiment 6 *d* for rendering the skin anæsthesic.

1. The Sense of Locality. Touch yourself in several places with the same object, and analyze out, as far as you can, the particular quality of the sensation by which you recognize the place touched. This quality of the sensation is known as the "Local Sign."

2. Cause the subject to close his eyes; touch him on the fore-arm with a pencil point; and require him to touch the same point with another pencil immediately afterward. Estimate the error in millimeters and average the results for a number of trials, noting the direction of error, if it is constant. The subject may be allowed to correct his placing of the pencil if not satisfied with it on first contact.

3. Aristotle's Experiment. Cross the middle finger over the first in such a way as to bring the tip of the middle finger on the thumb side of the first finger. Insert between the two a pea or other small object. A more or less distinct sensation, of *two* objects will result, especially when the fingers are moved.

4. Judgments of Motion on the Skin. *a*. Subject with closed eyes. Resting a pencil point or the head of a pin gently on the fore-arm, move it slowly and evenly up or down the arm. Require the subject to indicate his earliest judgment of the direction. If the experiment is carefully made, the fact of motion will be perceived before its direction. *b*. Try a number of times, estimating the distances traversed in millimeters and averaging for the two directions separately. It will probably be found that the downward distances have been greater than the upward. *c*. Starting from a fixed point on the fore-arm move the pencil in irregular order up, down, right or left, and require the subject to announce the direction of motion as before.

Cf. Hall and Donaldson, Motor Sensations of the Skin; Mind, X, 1885, 557.

5. Rest the fingers lightly on the forehead and move the head from side to side keeping the fingers motionless. Almost the whole of the motion will be attributed to the fingers. Light tapping of the forehead with the finger we feel in the forehead more markedly than in the finger. With our own hand on our forehead we feel the forehead; with some one else's hand we feel the hand.

6. Weber's Sensory Circles. *a*. Find the least distance apart at which the points of the æsthesiometric compasses can be recognized as two when applied to the skin of the fore-arm. Try also the upper arm, the back of the hand, the forehead, the finger-tip and the tip of the tongue. Be very careful to put both points on the skin at the same time and to bear on equally with both. *b*. Compare the distance between the points just recognizable as two when applied lengthwise of the arm with that found when they are applied crosswise. *c*. Give the points a slightly less separation than that found for the fore-arm (crosswise) and beginning at the elbow draw the points downward side by side along the arm. They will at first appear as one, later as two, after which they will appear to separate as they descend. Something similar will be found on drawing the points from side to side across the face so that one shall go above, the other below the mouth. *d*. Make the skin anæsthesic with an ether spray and test the discriminative sensibility as before.

Cf. Weber's measurements as given in the text-books.

7. Filled space is relatively under-estimated by the skin. Set up in a small wooden rod a row of five pins separated by intervals of half an inch, and in another two pins an inch and a half apart. Apply to the arm like the compasses above. The space occupied by the five pins will seem less than that between the two.

8. Active touch, that is touch with movement, is far more discriminating than mere contact. Compare the sensations received from resting the tip of the finger on a rough covered book with those received when the finger is moved and the surface "felt of."

9. The time discriminations of the sense of contact are very delicate. Strike a tuning-fork, touch it near the bottom of the prong and immediately remove the finger so as not to stop the fork. The taps of the fork on the skin do not blend into a continuous sensation for the tactual sense, even when the vibrations are 1000 or more a second.

For sensations of minimal contact, see Ex. 22.

On the foregoing experiments, cf. Weber, Tastsinn und Gemeingefühl, Wagner's Handwörterbuch der Physiologie, Vol. III. pt. 2; Funke, Hermann's Handbuch der Physiologie, Vol. III, pt. 2.

Sensations of Temperature.

Apparatus. Two brass rods (6 inches long and 0.25 inch in diameter, turned down to a fine smooth point 0.5 mm. in diameter), paper ruled in mm. squares, menthol pencil (such as is used for headaches), centigrade thermometers, vessels of water at different temperatures.

10. Hot and Cold Spots. *a.* Move one of the pointed brass rods, or even a cool lead pencil slowly and lightly over the skin of the back of the hand. At certain points distinct sensations of cold will flash out, while at others no temperature sensation will be perceived, or at most, only a faint and diffuse one. Heat one of the rods and repeat the experiment. *b.* On some convenient portion of the skin mark off the corners of a square 2 cm. on the side. Go over this square carefully both lengthwise and crosswise for both heat and cold, drawing the point along lines 1 mm. apart, and note on a corresponding square of millimeter paper the hot and cold spots found, hot spots with red ink, cold with black. This time the points should be heated or cooled considerably by placing them in vessels of hot or cold water and should be kept at an approximately constant temperature by frequent change, one being left in the water while the other is in use. Break the experiment into a number of sittings so as to avoid fatiguing the spots; for they are very readily fatigued. A map made in this way cannot hope to represent all the spots, but it will suffice to show the permanence of some of them and possibly to show their general arrangement. *c.* Notice the very distinct persistence of the sensations after the point has been removed.

11. The temperature spots respond with their characteristic sensations to mechanical (and electrical) stimulation, and do not give pain when punctured. *a.* Choose a very certainly located cold spot and tap it gently with a fine wooden point (not too soon after locating it, if it has been fatigued in locating); or better have an assistant tap it. *b.* Thrust a needle into a well located cold point. Try both for comparison on an adjacent portion of the skin.

12. The temperature spots respond to chemical stimulation. Choose a convenient area, say on the back of the hand, and take its temperature carefully, allowing the thermometer to remain in contact with the skin as long as it continues to rise. Note the temperature and rub the skin lightly with a menthol pencil. After a little the sensation of cold will appear. Take the temperature of the skin again; it will be found as high or higher than before, in spite of the contrary sensation. The menthol makes the nerves of cold at first hyperæsthesic (so that they respond with their specific sensation to mere contact, and give an intenser sensation when a cold body is applied than do adjacent normal portions of the skin); afterward, however, all the cutaneous nerves become more or less anæsthesic.

Cf. on the foregoing experiments: Blix, Zeitschrift für Biologie, Bd. XX, H. 2. 1884. Goldscheider, Neue Thatsachen über die Hautsinnesnerven, Du Bois-Reymond's Archiv, Supplement-Band, 1885, pp. 1–110; Donaldson, On the Temperature-sense, Mind, X, 1885; and the literature cited by these authors. On the chemical stimulation of the temperature nerves: (Cold) Goldscheider, Ueber die specifische Wirkung des Menthols auf die Temperatur-Nerven, Verh. d. Berliner physiol. Gesell. 9 April, 1886, Du Bois-Reymond's Archiv, 1886, p. 555; (heat) Die einwirkung der Kohlensäure auf die sensiblen Nerven des Haut, Verh. d. Berliner physiol. Gesell. 25, Nov. 1887, Du Bois-Reymond's Archiv, 1888.

13. The temperature of the skin at any moment is a balance between its gain and loss of heat. Anything that disturbs that balance, causing increased gain or loss of heat, produces temperature sensations. It is common experience that a piece of cloth, a bit of wood, a piece of metal, all of the same temperature as the air that seems indifferent to the hand, cause different degrees of the sensation of cold when touched, because they increase the loss of heat by conduction in different degrees. If a paper bag be placed over the hand held upward, a sensation of warmth is soon felt, because of the decreased loss of heat.

14. Provide three vessels of water one at 30° c., the second at 40°, the third at 20°. Put a finger of one hand into the warmer water, a finger of the other into the cooler. At first the usual temperature sensations will be felt, but after a little they disappear more or less completely, because of the fatigue of the corresponding temperature organs. Now transfer both fingers to the water of normal temperature. It will seem cool to the finger from warmer water and warm to the one from cooler.

15. The intensity of the temperature sensation depends on the amount of surface stimulated. Dip a finger in cold water, then the whole hand. Notice the increase in sensation.

16. The fatigue of the temperature apparatus may produce an apparent contradiction of Ex. 15. Dip one hand entirely under cold water and keep it there for a moment. Then dip the finger of the other hand or the whole hand several times in the same water, withdrawing it immediately each time. The water seems colder to the finger or hand which is only dipped.

17. Hold a very cold piece of metal on the forehead or on the palm of the hand for half a minute. On removing it the sensation of cold continues though the actual temperature of the skin is rising. Sometimes fluctuations are observed in the persisting sensation. After contact with a hot body the sensation of heat continues in the same way, though the temperature of the skin falls. Goldscheider explains this result for cold in part by the persistence of the cold sensation in the manner of an after-image, and in part by the lessened sensibility of the nerves of heat; a similar explanation *mutatis mutandis* holds also for heat.

18. Extreme temperatures fatigue the sensory apparatus of both heat and cold. *a*. Hold a finger in water of 45° c., the corresponding finger of the other hand in water which feels neither cold nor hot (about 32°). After 10 seconds dip them alternately into water at 10°. The finger from the water at 32° will feel the cold more strongly. *b*. Hold a finger in water at 10°, the corresponding finger of the other hand in water at 32°. After 10 seconds dip them alternately in water at 45°. The finger from the water at 32° will feel the heat more strongly.

19. Hold the hand for one minute in water of 12° c., then transfer it to water of 18°. The latter will at first feel warm, but after a time cold again. The water at 18° first causes a decrease in the loss of heat or a slight gain but later a continued loss.

20. Fineness of temperature discrimination. *a*. Find what is the least perceptible difference in temperature between two vessels of water

at about 30° c., at about 0°, and about 55°. The finest discrimination will probably be found with the first temperature, if the discrimination does not prove too fine at all these points to be measured with the thermometers at hand. Use the same hand for these tests, always dipping it to the same depth. It is better to dip the hand repeatedly than to keep it in the water. *b.* The different surfaces of the body vary much in their sensitiveness to temperature. The mucous surfaces are quite obtuse. When drinking a comfortably hot cup of coffee, dip the upper lip into it so that the coffee touches the skin above the red part of the lip, or dip the finger into it; it will seem burning hot. Plunge the hand into water at 5–10° c. The sensation of cold will be strongest at first on the back of the hand where the skin is thin, but a little later will come out more strongly in the palm, where it will continue to be stronger as it approaches pain.

On these general temperature experiments cf. the works of Weber and Goldscheider already cited, also Hering, in Hermann's Handbuch der Physiologie, Vol. III, pt. 2, pp. 415-439. Fechner, Elemente der Psychophysik, Vol. II, pp. 201–211.

Sensations of Pressure.

Apparatus. Bits of cork. Weights for minimal pressure. (These can be cut from rectangular prisms of cork or elder-pith of equal area, and provided with bristle or hair handles and verified upon a sensitive balance. The prism should be from 3 to 5 mm. square. The handle is made by setting the ends of a piece of bristle or hair into opposite sides of the bits of cork or elder-pith, thus giving the whole something the shape of a seal ring, of which the cork is the seal and the bristle the band. A series ranging from 0.002 to 0.02 grams would be convenient; but for the experiment to follow is not necessary.) Two objects of equal weight, but unequal size; a large cork and a small one, made of equal weight by loading the smaller with shot, answer very well. Two metal disks of equal size and weight, *e. g.* dollar pieces; and two wooden cylinders three quarters of an inch in diameter and one inch long. Vessels of water at normal temperature. Weights for discriminative sensibility. (The last can readily be made by loading paper gun-shells with shot. The following would be a convenient series: One hundred grams (two of this weight), 102.5, 103.3, 104, 105, 106, 107. The Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, England, manufactures a set, which can also be used for "muscle-sense" tests, containing 30 weights and giving ratios ranging from about one-fourth to one fiftieth, at a price of £5.)

On apparatus for sensations of pressure cf. Beaunis, Éléments de physiologie humaine, II, 579. Eulenberg, Berlin. klin. Wochensch. 1869, No. 44. (See illustration and description of Eulenberg's instrument in the Reference Hand-book of the Medical Sciences, Vol. I, p. 85.) Dorhn, Zeitschr. f. rat. med., 3 R., X, 337. Bastelberger, Experimentelle Prüfung der zur Drucksinn-Messung angewandten Methoden, Stuttgart, 1879. Jastrow gives in the American Journal of Psychology, II, 54, a very inadequate description of a very satisfactory instrument. See also notes on apparatus for the study of the Psychophysic Law to be given later.

21. Pressure points. Make an obtuse but extremely fine cork point (pyramidal in shape, for example, the pyramid a quarter of an inch square on the base and of equal height), set it upon the point of a pen or other convenient holder, or use a match whittled down to a fine point, or even a needle. Choose an area on the forearm and test for its pressure spots somewhat as for the hot and cold spots, but this time set the cork point as lightly as possible on point after point of the skin instead of drawing it along. Two kinds of sensation will be felt; at some points a clear feeling of contact with a sharp point will be felt, at others no feeling at all or a dull and vacuous one. The first are the pressure points. Goldscheider describes their sensations on light contact as "delicate," "lively," "somewhat tickling * * * as from moving a hair;" on stronger pressure, "as if there was a resistance at that point

in the skin, which worked against the pressure stimulus;" "as if a small hard kernel lay there and was pressed down into the skin."

The first are more sensitive to small changes of pressure, and, though with sufficient increase both give pain, their sensations retain their characteristics. They are closer together than the temperature spots, and harder to locate; and the fact that our most frequent sensations of pressure are from surfaces and not from points makes it difficult at first to recognize a pressure quality in their sensations.

Cf. Goldscheider, Neue Thatsachen über die Hautsinnesnerven. DuBois-Reymond's Archiv, 1885, Supplement Band, pp. 76–84.

22. Minimal pressure (or simple contact). Make weights that are just perceivable on the volar side of the fore-arm and on the tips of the fingers. Try also if convenient the temples, forehead and eye-lids. In applying the weights see that they are brought down slowly upon the surface of the skin, that they touch equally at all points, and that their presence is not betrayed by motion of the weight after it touches the skin. This can be done by using a pen-holder or small rod, with its tip put through the ring of the weight, for laying it on. Compare the relative sensibility found by this method with that found with Weber's compasses for the same parts, and note that the latter requires discrimination, not mere perception. The stimulus needed to produce this faintest sensation is known as the stimulus of the "Initial Threshold." See also experiment 28.

Cf. Aubert and Kammler, Moleschott's Untersuchungen, V, 145.

23. Relation of apparent weight to area of surface stimulated. Test with the equal weights of unequal size. The smaller will seem decidedly heavier.

24. Discriminative sensibility for pressures. Have the subject lay his hand, palm upward, on such a support as will bring his arm into a comfortable position and make his palm level, for example a folded towel on a low table or the seat of a chair. (The matter of an easy position for the subject is of cardinal importance in all psychological experiment, and is mentioned here once for all.) Lay in his palm a piece of blotting paper just large enough to prevent the weight from touching the skin. On this set a standard weight, *e. g.* 100 grams, and after a couple of seconds replace it with an equal weight, or one heavier (or one lighter) *e. g.* 106 grams, allowing that to remain an equal time. Require the subject to say whether the second weight is the same or heavier, (or lighter, if a lighter is being used). Find the weight that can be distinguished from the standard in 75 per cent. of the trials. The ratio between the difference of these and the standard is the index of the sensibility. The ratio will probably be about 5 : 100. It is best not to use both a lighter and a heavier in the same series; and with this method of testing the subject should always guess, if he cannot discriminate. Be careful in putting on the weights that the subject does not recognize a difference in the force with which they strike, also that suggestions by difference of temperature or by sounds made in selecting the weights are avoided. This method of determining sensibility is known as the "Method of Right and Wrong Cases." Cf. later experiments on the Psychophysic Law.

25. Cold or hot bodies feel heavier than bodies of equal weight at a normal temperature. *a.* For cold, take two dollar pieces, warm one until it ceases to seem cold; cool the other to 10° c. Apply alternately to the palm of the hand. The cold one will seem much heavier, perhaps as heavy as two at the normal temperature. *b.* For heat, take two wooden cylinders of equal weight, heat one to a high temperature by standing it on end in a metal vessel floating in a water bath. Apply the cylinders on end alternately to the back of the hand between the metacarpal bones of the thumb and first finger. The hot one will seem heavier.

26. Pressure evenly distributed over a considerable area is less strongly felt than pressure upon an area bordered by one that is not pressed. Dip the hand up to the wrist into water (or better still into mercury) of normal temperature and notice that the sensation of pressure is strongest in a ring about the wrist at the surface of the water, possibly stronger on the volar than on the dorsal side. The ring effect is unmistakable when the hand is moved up and down in the water.

27. Something of the refinement of the pressure sense in perceiving the unevenness of surfaces may be seen by laying a hair on a plate of glass or other hard, smooth surface and over it 10 or 15 sheets of writing paper. The position of the hair can easily be felt by passing the finger tips back and forth over the surface.

28. Something might be said in support of the hairs as independent sense organs. The finest respond with a distinct sensation of anticipatory touch, as it were, when they are moved, and probably this accounts for a part at least of the differences between the fore-arm and finger tips found in Ex. 21. Touch a few single hairs and observe the sensation.

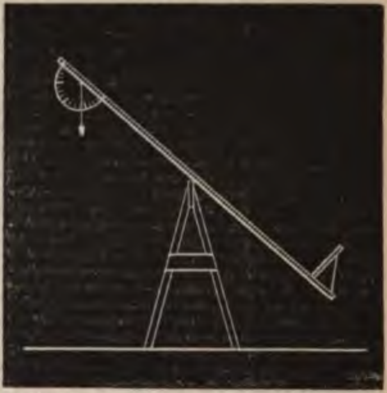
Cf. Blaschko, Zur Lehre von den Druckempfindungen, Verhandl, d. Berliner physiol. Gesell. Sitz, 27 März, 1885, DuBois-Reymond's Archiv, 1885, p. 349.

On the general topic cf. Weber, *op. cit.*: Funke, Tastsinn und Gemeingefühl, Hermann's Handbuch der Physiol., Vol. III, pt. 2, pp. 289-414.

II.—STATIC AND KINÆSTHESIC SENSES.

This group of senses furnishes us with data respecting the positions and motions of our members, and of our bodies as wholes. It includes senses whose existence or efficiency is disputed *e. g.* (Innervation Sense and Muscle Sense) and others whose independence has lately been asserted (*e. g.* Joint Sense and Tendon Sense.) This embarrasses somewhat the selection of experiments, but those chosen are the ones that seem at present characteristic. Many of the weightiest psychological inferences depend upon the sensations of motion and position of the eyes. It seems best, however, to postpone the experiments upon these sensations to the section upon vision.

RECOGNITION OF THE POSITION OF THE BODY AS A WHOLE.



Apparatus. A light wooden rod a yard long; a tilting board and straps. For the last a board seven feet long and 18 inches wide balanced across a saw-horse will answer. At one end a foot board should be fastened securely enough to bear the weight of a man when the board is in a vertical position. At the other end a plumb line and semicircular scale should be added so that the inclination of the board can be read off at any instant. For holding the subject securely upon the board when its inclination is considerable, and the subject is head downward it will be necessary to have a couple of shoulder straps passing over the subject's shoulders and fastening to stout screw-eyes screwed into the board itself or into the foot board, and perhaps a breast strap going about both the subject and the board.

Cf. Aubert, Physiologische Studien über die Orientierung, (translation with comments of Delage's Études expérimentales sur les illusions statiques et dynamiques de direction, etc.,) Tübingen, 1888, p. 41.

29. In this experiment it is especially desirable that the subject should know as little as possible of the purpose of the experiment. Cause the subject to stand erect with his back against a wall. Choose a point on the opposite wall about the height of his shoulders. Let him look at it, and then require him, having closed his eyes, to point to it as exactly as possibly with a light rod held symmetrically in both hands. Cause him also to hold the rod vertically and horizontally in the median plane; also horizontally parallel to the frontal plane. All these he will probably be able to do with much accuracy, or if, as sometimes happens, he shows a "personal equation," his error will be constant.

30. *a*. Cause the subject to repeat the experiment, this time turning his head well to the left after closing his eyes. Repeat, causing the subject to turn to the right. In both cases an error of about 15° will be observed, the subject pointing too far by that amount in the direction opposite to that of the turning of the head. The subject will be able to hold the rod vertical or horizontal without error. *b*. Cause the subject to hold the rod in what he thinks is a horizontal position in the median plane, when his head is thrown well back; when bowed well forward. Illusions like those observed above will result. *c*. Cause the subject to hold the rod in what he thinks is a horizontal position, parallel to the frontal plane, when his head is leaned to the right; when leaned to the left. Illusions similar to those in the previous experiments will appear. *d*. Repeat experiment *a*, but instead of having the subject point to the designated object, have him walk toward it keeping his shoulders square, his eyes shut, and his head turned to one side. He will walk more and more too far toward the side away from which his head is turned. In all these cases judgment of one cardinal direction in space alone is affected, the other two show little or no errors.

31. After some practice and with attention to the sensations, the illusion of Ex. 30 takes another form, namely, that the body has turned a few degrees in the same direction as the head. The subject can now point to the chosen object; but, if required to set the end of the rod against his breast so that it shall be horizontal and perpendicular to the line joining his shoulders, he will make an error of about 15° in the direction of the motion of the head. A similar illusion may be found for the other directions of head turning, if tried under proper conditions *e. g.* when hanging by the hands with the arms somewhat bent.

32. The illusion is due, at least in cases *a* and *b* Ex. 30, to sensations of the position of the eyes. As may easily be observed upon any other person, the eyes turn further than the head in the direction in which it is turned. From the eyes we judge the position of the head, and thus overjudging it,point too far in a contrary direction in trying to point to the required object. The illusions can be produced by motion of the eyes alone. *a*. Holding the head erect and taking pains not to move it when moving the eyes, turn the closed eyes as far as possible to the right or left and then try to point to some determined object. An error like that in Ex. 30 will be observed. Turning of the eyes upward or downward has a less satisfactory result. Instead of closing the eyes they may be kept open if an opaque screen is held close before the face. *b*. Repeat *a* and *b* of Ex. 30, voluntarily turning the eyes as far as possible in the direction opposite to that of the turning of the head. The original error will disappear or be found to have changed its sign.

33. Another set of illusions regarding the position of the body as a whole in space depend in large measure on the distribution of pressure on the surfaces of the body, the direction of pressure of the movable viscera and the blood. Secure the subject properly upon the tilting board, and have him close his eyes. Start with the board vertical, (head up).

The subject will probably announce that he is then leaning forward slightly. Turn him slowly backward and require him to announce when he is vertical (head up), when he is tilted backward at an angle of 45° from the vertical, when at an angle of 60°, when at 90°, when at 180°. Two classes of illusions will be found; angles of less than 40° will seem too small; those from 40° to 60° will be rightly judged; those beyond 60° will seem too large. The subject will probably say that he is vertical, head downward, when he is yet 30-60° from it.

SENSATION OF ROTATION.

Apparatus.—Rotation Table. This can be made well enough for the experiments given by laying a 7-foot board across an ordinary turning chair or screw stool without a back. The last must turn without appreciable noise or jar. Many of these experiments could be made perfectly well by twisting the ropes of an ordinary swing.

34. Lay the board across the stool and let the subject be seated upon it with closed eyes and blindfolded if necessary. Turn the stool slowly and evenly in one direction or the other. The subject will immediately recognize the direction and approximately the amount of rotation when the rate is as slow as 2° per second, or even slower. After continued rotation at a regular rate the sensation becomes much less exact or entirely fails. This fact has been generalized by Mach in the law that only change of rate, not continuous rotation is perceived. With an exception in the case of uniform rates for short times, this is accepted by Delage. After some pauses and short movements in one direction and the other, the subject may become quite lost and give a totally wrong judgment of the direction of motion, if it is slow.

35. Let the subject be seated as before. *a.* Rotate him a little more rapidly for half a turn, and then stop him suddenly. A distinct sensation of rotation in the opposite direction will result. *b.* Repeat, and when the illusory rotation begins, open the eyes. It immediately ceases. Close the eyes again and it returns.

36. *a.* Repeat experiment 35 *a*, letting the subject give the word for stopping. At the same instant let him incline his head suddenly backward or forward or lay it upon one shoulder or the other. The axis of rotation of the body will appear to change in a direction opposite to that of the inclination of the head, i. e., if the head is inclined to the right, the axis, seems to incline to the left. The feeling is as if the body were rotating in the surface of a cone in a direction contrary to that of the first rotation. The head dictates the apparent axis of rotation. The same illusion occurs if the head is inclined during the actual rotation and straightened at the word for stopping. Turning the head to right or left introduces no such illusions, because it does not change the axis of rotation of the head. The illusion comes out with very disagreeable strength when the rotation is rapid and the subject changes the position of his head during the rotation. *b.* Let the subject lie upon his side and rotate him rather rapidly till the sensation of rotation becomes faint or disappears. Then let him turn suddenly upon his back or upon his other side. The first brings the rotation about a new axis, and it is felt in its true sense, while the rotation about the previous axis is felt in its reverse sense; the second reverses the direction of motion completely and produces a correspondingly powerful sensation.

The change of the apparent axis of rotation with the change of position of the head points to the location in the head of the organ by which such sensations are received. For the experiments by which the semicircular canals are indicated as this organ see the literature cited below.

37. Another illusion of rotation (Purkinje's dizziness) is due to the

motion of the eyes. Let the subject whirl rapidly on his heels with his eyes open till he begins to be dizzy; while he whirls the objects about him will seem to be turning in the opposite direction. Let him then stop and look at an even surfaced wall while the experimenter carefully observes his eyes, picking out a fine blood-vessel, or some other clearly marked fleck or spot as a point at which to look. To the subject the surrounding objects will seem to continue to move in the same direction as before, i. e., in a direction contrary to his previous rotation; the experimenter will see the subject's eyes executing slow motions in one direction (in the direction of the original motion of the subject) alternating with rapid motions in the other. The subject himself may be able to perceive a corresponding irregularity of motion in the spots upon the wall at which he looks. The illusion rests upon the subject's unconsciousness of the slow motions of his eyes. It is not improbable that these eye motions and the sensations of attempted restoration of equalibrium in other parts of the body are reflexly caused by the disturbance in the semicircular canals. It should be noticed that this illusion is the exact reverse of that found with closed eyes in Ex. 35. There the subject feels a rotation of his own body contrary to that it previously received. If he was turned at first in the direction of the hands of a watch, on being stopped he would seem to be turning in a direction contrary to the hands. If these motions were transferred to objects about him, they would, during the rotation, seem to move contrary to the hands and after stopping in the direction of the hands. In the Purkinje experiment the motion of objects is not thus reversed.

SENSATION OF PROGRESSIVE MOTION.

39. So far as progressive motions do not partake of rotation the sensations which they give us are probably combinations of sensations from several different sources or sensory judgments based thereon. For them, as for the motions of rotation, the principle holds that we perceive changes of rate of motion, and not uniform motion; as long as the motion remains uniform we can by an effort of imagination conceive ourselves to be moving in either direction or to be standing still, except for the jarring. The apparatus for the study of these phenomena will be found in railroad trains and elevators.

On the sensations of this and the preceding sections, cf. Aubert, translation of Delage above cited; Mach, Bewegungs-Empfindungen, Leipzig, 1875; Brown, On Sensations of Motion, Nature, vol. XL., 1889, p. 449, ff.

MUSCLE SENSE, *Kraftsinn.*

The real muscular sensations are probably those of pain, fatigue and the like, and are of relatively minor importance for psychology, but the term "muscle sense" has been used to designate that sense by which lifted weights are perceived, and is here used in that sense.

Apparatus. Set of test weights somewhat like those used for the pressure sense, but less different one from another, (For example: 100 grams (two of this weight), 101.6, 102, 102.2, 102.5, 102.8, 103,3). Weight of 2 or 3 kg.

39. Discriminative sensibility for lifted weights. *a.* Let the subject stand at a table of convenient height. Place within easy reach of his right hand, and near together, one of the standard weights (*e. g.* 100 gm.) and a weight to be compared with it, either the other standard or a heavier (or lighter) one. Let the subject lift one after the other, taking care to lift them in the same way, at the the same rate and to the same height, and give a decision as to which is the heavier (or the lighter). Find the weight that can be distinguished from the standard in 75 per cent. of the trials. As before the ratio between the difference of these and the standard is the index of the discriminative sensibility. The ratio will probably be about 2.5 : 100. *b.* Repeat the experiment letting the subject lift with one hand the standard and with the other

the weight to be compared, keeping the same hand for each during each series of trials. Note the discriminative sensibility as before; the discriminations will be much less fine.

In these experiments the sense of pressure might be expected to co-operate, but when it is excluded or put at a relative disadvantage, the sensibility for differences of lifted weights is not diminished. Weber's method of excluding the pressure sense was to wrap the weights in pieces of cloth and lift them by the four corners together. The pressure on these corners can be changed at will irrespective of the heaviness of the weight lifted. Compare the descriminative sensibility found for pressure with that found for lifted weights.

40. Careful experiments on the method of such discriminations shows that the determining factor is the rapidity with which the weight rises as it is lifted. The following experiment is one of those upon which this conclusion rests. After having performed the second part of Ex. 39, compare the standard weight with a very much heavier weight, *e g.*, 2 kg., with all the circumstances of actual careful judgment. Practice this judgment thirty times, leaving a larger interval of time between the individual comparisons than between liftings of the weights compared. Then at once return to the smaller weights, giving the standard to the same hand as before and the weight to be compared to the hand that has just been lifting the 2 kg. Not only will the weight before just recognizably heavier seem considerably lighter than the standard, but also still heavier weights will seem so. This time the tests must be few, not more than three or four. If more should be desirable, practice the comparison, of the standard and 2 kg. weight again ten times before taking them. By the practice the nervous centres discharging into the the muscles that raise the 2 kg. weight become accustomed to a larger discharge than that required for the small weights and do not at once re-adapt themselves, but supply too great a discharge, the weight rises with greater rapidity than the standard and is consequently pronounced lighter.

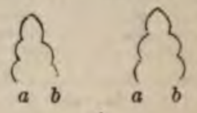
Cf. on Muscle Sense. Weber, *op. cit.*; Müller und Schumann, Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte, Pflüger's Archiv, Bd. XLV, 1889, pp. 37-112; James, Psychology, II, pp. 189 ff.

Cf. also the experiments and references on the Psycho-physic Law.

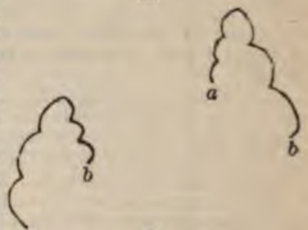
INNERVATION SENSE.

Apparatus. Black board and chalk.

41. The evidence most frequently offered in support of this sense is clinical and therefore beyond the scope of this course. Experiments like the following have been brought forward, but their interpretation has been disputed. *a.* Stand erect before the black board with the eyes closed and coat off, if it interferes with free motion of the arms. Draw with each hand a conventional leaf-pattern like those in the annexed cut drawing from *a* to *b* in both cases. In drawing try to make the lobes of the leaf of equal size, like those in Fig. *A*; draw each with a single simultaneous "free hand" motion of the arm, that is, draw each with a single volitional impulse directed equally to the two sides—the last point is important. First draw a pair of leaves beginning them with the hands before the shoulders at the same height; the result will be approximately like fig. *A*. Next draw a pair with one hand about a foot lower than before; the result will be like Fig. *B*. *b*. Bring the hands again



A



B

to the position used in drawing fig. *A*, and draw a pair of leaves having their apices right and left. The leaves will be symmetrical. Next begin with one hand about a foot farther away from the median plane than before and the other at it, but both at the same level. Draw as before; asymmetrical leaves will be the result. Repeat the drawing a number of times, sometimes raising or extending one arm, sometimes the other. In general it will be found that notwithstanding the intention to make equal movements of the hands, the motions of further extension in the extended arm and of further flexion in the flexed arm are too short and those in the contrary direction in each case too long. The argument founded on this experiment runs as follows: We think that our hands execute equal movements, when they do not, because we are conscious of willing equal movements, and unconscious or only inexactly conscious of those actually made. If on the contrary we perceive motion of members by the skin, joint and muscle sensations that accompany their motion (as the opponents of the Innervation Sense believe) we ought to know the extent to which our hands are moved each time and not fall into the illusion that we find in these experiments.

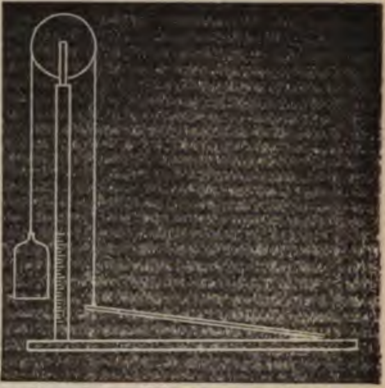
42. Lay the hand palm downwardon the edge of the table or on a thick book so that the last three fingers shall be supported and held extended while the thumb and first finger remain free. Bend the first finger considerably at both the inner joints, and hold it in position with the other hand. The finger tip is still movable as will be found on touching it, but it is anatomically impossible to move it voluntarily. When, however, the effort is made to move it (the eyes being closed) there is a sensation of motion, though no actual motion is possible.

43. Of experimental evidence against the Innervation Sense there is more. Müller's experiment (No. 40) seems conclusive against it; for if there were any sensation of nervous discharge, we ought to know that when we go from a very heavy to a light weight the discharge is disproportionate; but we do not. That the feeling of effort is of peripheral and not central origin is shown by such experiments as this of Ferrier's. *a*. Hold the finger as if to pull the trigger of a pistol. Think vigorously of bending the finger, but do not bend it; an unmistakable feeling of effort results. Repeat the experiment and notice that the breath is involuntarily held, and that there are tensions in other muscles than those that would move the finger. Repeat the experiment again, taking care to keep the breathing regular and other muscles passive. No feeling of effort will now accompany the imaginary bending of the finger. *b*. Lay the fore-arm entirely relaxed in the scale pan of an ordinary balance (or better still of a spring balance) and put in weights enough to compensate it exactly. Remain with closed eyes keeping the arm relaxed. It will after a little overbalance the compensating weights, showing that at first it was not wholly relaxed. An Inervation Sense, if we had one, ought to prevent such an illusion.

Cf. on the Sense of Innervation, Wundt, Physiologische Psychologie; I, 397, ff.; Sternberg, Zur Lehre von den Vorstellungen über die Lage unserer Glieder, Pflüger's Archiv, XXXVII, 1885, 1. Loeb, Untersuchungen über die Orientirung im Fühlraum der Hand und im Blickraum, Pflüger's Archiv, XLVI, 1—46, (but see also criticisms of James, Psychology, II, 516, and of Christine Ladd Franklin, Amer. Jour. Psy., II, 653); James, Psychology, II, pp. 486, ff.; Ferrier, Functions of the Brain, pp. 382 ff., (English Ed.); Funke, *op. cit.*

SENSATIONS OF MOTION, (*Joint Sense*).

Apparatus. Hinged board for passive flexion of the elbow. The accompanying cut will give some idea of the construction of such a board. The thin board on which the fore-arm rests (50 cm. long by 8-10 wide) is hinged at one end to the base board. At the other end a cord is fastened that runs over a pulley upon the top of a stout post. On the end of the cord a weight is hung to counterbalance the weight of the fore-arm. A scale (*e. g.* a piece of mm. paper) on the post near the weight enables the experimenter to read off the distance which the end of the arm-board is raised or lowered. It is essential that the hinge and pully work easily and without jar. The above is simply one way of accomplishing the result; others will occur to those for whom this construction is inconvenient.



44. Passive flexion at the elbow. Let the subject rest the fore-arm flat upon the arm board, bringing the elbow over the hinge, and close his eyes; raise the fore end of the arm-board slowly by pressing down upon the counter weight, and require the subject to announce when he first perceives the *motion* of his fore-arm. It is extremely important not to mistake the sensation of increased pressure or of jar for that of motion. With the dimensions given above, one degree of angle corresponds to about 8.7 mm. The same apparatus may be used for extension as well as flexion.

45. Active flexion of the last joint of the finger. The joint sensations of the fingers are less fine than those of the elbow, but are more convenient for demonstration of active flexion. Fasten a piece of straw, with court-plaster or otherwise, to the finger nail of the middle finger, and cut it off at such a length that the distance from the joint of the finger to the end of the straw shall be 115 mm. With that radius 2 mm. corresponds to about 1° of angular measure. Rest the hand on a thick book letting the last joint of the finger extend beyond the edge. Set up a millimeter scale at right angles with the straw. Close the eyes and make the least possible flexion of the finger at the last joint, having an assistant note its extent on the scale. Between one and two degrees will probably be the least possible voluntary movement. Close attention will, probably in both these cases, locate the chief sensation in the joint. For the more rigorous experiments required to show its character clearly and to prove its location see the following:

Goldscheider, Untersuchungen über den Muskelsinn. Du Bois-Reymond's Archiv, 1889, pp. 369 ff. and 540, also Supplement-Band, 1889, 141 ff.

SENSATIONS OF RESISTANCE.

Apparatus. Two or three kilogram weight and string. Vessel of mercury.

46. *a.* Hold the weight by the string so that it hangs a few inches above the floor, with the arm extended. Lower the weight rather rapidly till it rests on the floor. As it strikes, an illusion of resistance to further motion will be perceived. This is due to the unexpected strain put upon the muscles that lower the arm by the tension of those that have been holding the weight. The feeling of resistance is probably a

joint-sensation. *b.* Something similar is observed on pouring a quantity of mercury from one vessel to another.

Cf. Goldscheider, *op. cit.*

BILATERAL ASYMMETRIES OF POSITION AND MOTION.

Apparatus. Two medium sized corks. A millimeter scale at least one meter long. This can easily be made by pasting millimeter paper upon a smooth wooden slat. A convenient scale has a right angled triangular section. In use this stands upon the short side of the triangle, the long side is next the subject, the hypothenuse next the observer. The millimeter paper is pasted along the upper edge of the side next the observer.

47. Apparently symmetrical positions of the two arms. Hold a cork between the thumb and first two fingers of each hand. Close the eyes and bring the two corks together at arms length in the median plane before the face, having an assistant note the approximate amount and direction of the error. The corks should be brought together rather gently so as not to betray the character of the error to the operator, but the motions of the arms by which they are brought up nearly to contact should be free and sweeping. The error will probably be found rather constant in direction until the operator learns to correct it. Try bringing the corks together above the head, and also in asymmetrical positions.

48. Let the subject seat himself at a table with the millimeter scale before him. Set a pin in the middle of the scale and bring the pin into the median plane of the subject and make the scale parallel to his frontal plane. Let the subject place his forefingers on either side of the pin, and with closed eyes, try to measure off equal distances by moving each outward along the scale. Note the result in millimeters; for this it may be convenient to mark the middle point of the finger-nails with an ink-line. A constant excess in the motion of one hand or the other will be found. It is important that the subject should not open his eyes till his fingers are removed from the scale; for he will find it difficult not to correct his error if he knows its nature. The finger tips should rest lightly on the scale and the motions should be made by a single impulse; if they are too slow and the subject attends to his sensations of position, the errors will be small and uncertain. The greatest errors will probably be found for distances of 20 to 50 cm. from the median plane. The left hand generally makes the greater excursion in right handed persons not mechanics. *b.* Repeat the tests having the motions of the hands made successively instead of simultaneously. The constant difference between the hands will not appear. c. Operate somewhat as in *a*, but this time let the experimenter move one of the subject's hands passively while the subject himself tries to move the other at the same rate and to stop instantly when the passive motion stops. Try passive motions of the right as well as the left hand. The errors found will generally resemble those of *a*. *d.* Let the subject start with his right or left hand 20 cm. toward its own side of the median plane, and try to measure off equal distances on either side of that point, using the same hand for both distances. Indicate the point of departure with a pin as before and mark off with another the standard distance to be reproduced. Distances outward will be made too large, distances inward too small. In all these experiments with closed eyes we seem inclined to judge distance rather from the intention of equal motion and the continuance of motor sensations for equal times than from the actual peripheral sensations.

Cf. Hall and Hartwell. Bilateral Asymmetry of Function. Mind. Vol. IX; Loeb, Pflüger's Archiv, XLI, 1887, pp. 107-127, also Pflüger's Archiv, XLVI, 1890, pp. 1-46.

(*To be Continued.*)

CONTEMPORARY PSYCHOLOGISTS.

I.

PROFESSOR EDWARD ZELLER.

Professor Zeller was born in 1814. At the age of twenty-six he became docent of theology in Tübingen, in 1849 was called to Berne as assistant professor, and to Marburg as full professor two years later. In 1862 he was called to Heidelberg and transferred from theology to the chair of Philosophy, and ten years later came to Berlin as the successor of Trendelenburg, where he is now the leading teacher among six in his department. Here, in 1878 he was elected rector, receiving, according to the liberal German custom, a large fraction of all the examination fees. His chief publications are, *Geschichte der Griechischen Philosophie*, the first edition of which was begun in 1844, the fourth in 1876; *Vorträge und Abhandlungen*, in two volumes, 1875 and 1877, and *Geschichte der Deutschen Philosophie seit Leibnitz*, in one volume, 1873. He was the founder and for many years the editor of *Die Theologische Zeitschrift*, and has written shorter articles, to be found only in its pages. Personally he is a man of moderate height, almost alarmingly thin, sallow, of distinct, deliberate monotonous delivery, of genial but somewhat precise manners in personal intercourse. His lectures attract hundreds of students each semester. As a comparatively young man he married the daughter of F. C. Baur of Tübingen, and his family relations have been exceptionally happy. He, with several others of the prominent professors of Berlin, was a frequent and familiar guest of the family of the Crown Prince, Friedrich, and according to common report, was in an informal way the religious adviser of his consort. Next to Lotze he has been, no doubt, the most influential professor in his department in Germany. He has no system whatever, but has devoted his life to the critico-historical presentation of the views of others—a department of labor to which nearly half of all the philosophic writings and philosophic lectures in the German Universities are now devoted. In this work he is probably without a peer.

We shall first epitomize for our readers his remarkable summary of recent investigations in the field of comparative religions and the psychology of religion in the newly edited two volumes of essays, which contain his most personal as well as, perhaps, original views. In a laboriously compacted essay on the Origin and Essence of Religion he premises that philosophy, or psychology is the closest and most indispensible friend of religion. Doubt, which marks the important moment when we question or seek explanation about things which have surrounded us from childhood, is sure to lead to a deeper and purer faith, if it is manfully faced. The notion of God is not innate but is given perhaps necessarily, although always mediately, through experience, and thus rests upon the same sort of grounds and inferences as the notion of atoms, and is as scientific. In fact all intellectual possessions whatever are self-won, *we make* all which comes into our minds out of experience. All men irresistably infer that there are laws and that things are connected, not merely in time and space but inwardly; they not merely *are*, but *hang* together, and that not only in our ideas objectively carried over into experience but in themselves by the law of causation. Like all things else, causality itself must have a cause.

Thus at first as many substances are postulated as there are forces observed; later these forces are found so closely connected, their actions and reactions so poised and equipolated, the one by the other, that they must be thought as one. But this unity is, of course, as hypersensuous and immaterial as force itself. Consciousness in its essence is a union of the manifold, and hence by the very form of its knowing, its every object, whether cosmic or microcosmic must be a unity of the composite or the simple. Hence the necessity of inferring a primitive unitary force.

In illustration of these principles he proceeds to group a great number of facts gathered from popular psychology and mythology. From just such feelings as arise vividly in our own consciousness when we take the first spring ramble in the country, by the sea, or under the stars, from just such instinctive needs as we now feel in the thunderstorms, floods, in battle, famine or in sickness, and from the experience of common family and social life, not only all poetry and mythology but all religion have taken their origin. These are the feelings and needs they interpret. The strongest and most unwonted impressions drive us irresistably beyond the limits of our own ability or knowledge to supernatural causes. General forces are not deified. There are no gods of space or gravity. There was no interest in the beautiful but only in the pleasant and the fearful; and what was more fearful than night before man was possessed of fire. Because most of his instincts are satisfied, man assumes that somehow his wishes also must be, and so, when thought asks, fancy answers questions, though the answers are but changed facts. Thus the first religions are polytheistic and preserve a faithful record of the wishes, fears and hopes of prehistoric peoples whose spirits were wrought upon by nature and history with an intensity inversely as their knowledge of them. As long as no one dreamed that, e. g., even the sun and fire might be the same, no one thought of ordering this crowding and increasing throng of deities. But as men saw unity in the world and later as they learned to concentrate their own efforts upon some one paramount object, did first some group of gods become paramount, and later some one god was made king or president in the council of the others. Sometimes a tribe who had developed a national deity like that of the Jews induced others by argument or by the sword to accept it as their own. Perhaps monotheism was first the result of a philosophical critique, as among the disciples of Xenophanes, or again unity was favored or suggested by the all-enclosing arch of heaven and the apparent limits of the horizon and the visible universe. The visible always precede the invisible gods, and monotheism is always developed from polytheism. The personification of active forces is in fact the natural form under which the idea of cause is first represented. It is impossible to follow all the subtle associations of ideas by which the wind was represented as a breath, the lightning as God's spear, every bright or smooth stone as an amulet, the stars as living beings, by which a stick was thought to reveal hidden treasure, or a backward look to the left to cure sickness. Whether it was at first some fancied analogy or a rare but opportune chance sequence in time to which they owe their origin, they have possessed the mind of man for unnumbered ages, and hence, and because they minister, however unworthily, to a real need they are very hard to eradicate. As long as there is no postulate that all things are connected by laws, and in proportion as man is uncertain of his own position in an unexplored world where the most unexpected thing may happen to him at any moment, so long and so far is he anxious to get the unknown powers on his side by presents, wounds self inflicted, the offering of animals and finally of human beings and especially of innocent persons. Sometimes the deity is small and weak, or perhaps the priest, who even in the catholic church holds a sort of conjuring power, has found some magic liturgy or ceremony of mighty and constraining cogency and the god may even be whipped or imprisoned till the wish is gratified or the fear allayed.

But religions must ever become more rational, i. e. immaterial. This process begins before all known developed languages, in the inference that soul and body are distinct, and that the *ego* is not a whole, made up of soul and body as subordinate unities. In the sight of a dead acquaintance and in dreams this distinction begins. The souls of men are at first located in the heart, head, bowels, liver, diaphragm, etc., and are weak and shadowy. The "he himself" is the body, as in Homer. They lead an unreal life under the earth, affecting the fruits of the fields, are figured as ghosts, manes, elves, and finally, in German mythology, survive as dwarfs, till the conception took more definite form in the idea of Hades. At first only chiefs and leaders had souls and these are invoked along with the sun and moon as in the worship of ancestors.

As long as their favor could be bought deities had, of course, no moral character. As hospitality, agriculture, commercial and family life, conceptions of regularity in nature, etc., were developed, the gods ceased to be awe or fear-inspiring (*unheimisch*) and influenced more directly the life men. Religion now began to grow in importance with the worth of the moral life. The lowest form of prophecy is the interpretation of signs. Faith is the child of the wish; then an audible voice must speak from the clouds; then men are inspired by a daimon, which is the inner oracle of conviction below distinct consciousness. They are suddenly and perhaps violently possessed by ideas, which wrap them in a dream, revery or vision, so new and so absorbing that the poet or musician dare not claim his work as his own. They did not know the inner process but were sure of its results, and thus inspiration was the best form which creative genius knew to give itself.

From the above principles of its growth we may infer why religion sometimes became largely political as among the Jews; why ancient systems lay great weight upon the cultus or worship and modern upon dogmatic orthodoxies; why religions must change; why the tension between the old and the new is proportional to the rapidity of this change, why all that cannot be harmonized to the new standpoints should, as Schleiermacher said, be allowed to lapse from the Christian consciousness and above all we can see that, as we should not study the Bible to know what Jesus believed but what we must,—so the worth of religion does not depend on how it originated but on what it does, just as the dignity of man and of science are not impaired by the conviction that one was developed from the ape and the other from astrology, alchemy, etc., or as the cogency of a man's logic is not prejudiced by the fact that a few decades ago we could use only baby-language. Not the first form but the historic principle is the essential thing. No man loses esteem for the German people of today by reading Tacitus or studying the life of the mediaeval Teutonic tribes.

A poetic race will emphasize the mythological element, a speculative people the dogmatic and a practical the active side of religious life, but Schleiermacher was right in seeking a deeper common principle. We must always reason from what the religious consciousness *says* to what it *means*. Dogma ignores scientific interests. Knowledge is no measure of the worth of religious life and is valueless for its own sake. Morality, very far from being the natural and implacable enemy of religion, as Feuerbach argued, is its chief, but not only constituent. As well argue against the use of fire on account of conflagration or against civic life because of corruption, or against judicial tribunals because they sanctioned the torture chamber, as to reject religion because of the selfishness, fanaticism and superstition which are ever found to attend it. Religion it not merely recognition of our duties as divine commands, any more than the notion of God originated in the moral sense, but it includes everything which concerns the well being of man. It rests upon and is determined by the needs of social and individual life and especially of the *Gemüth*. Its cultus is the natural expression of a natural feeling and must evoke worthy frames of mind and all noble resolves. Does it bring joy and certainty into the life of the soul, does it increase the sense of personal happiness, rest, peace, etc., and not does it make us work successfully for the rewards of a future pay-day, are the questions? Can we dispense with the sense of our universal relationship and give up the postulate of the search for a unity of all things?

In his religious views Zeller has been greatly influenced by Schleiermacher whom he very justly terms "the greatest of all protestant theologians," a many-sided, Platonic mind, a true ethical genius who *must* preach," and who is best understood not by his avowed pupils but by those who, refuting his letter by his spirit, his later by his earlier works, have passed beyond his standpoint. His great object was to mediate between supernaturalism and rationalism, mysticism and empyricism, docetism and ebionetism, manicheism and pelagianism, to test the true value of all knowledge by the religious consciousness, to bring the culture of his time back to piety. In his earlier writings Schleiermacher argued that scripture became Bible solely by force of its own inward excellence, by the natural law of survival, in short that christianity does not insist upon being the only form of religion, but would prefer at any time to yield to a better should it appear. Not only all dogma but even Jesus himself, to whose person he attached such central importance later, but who never claimed to be the only mediator, were not indispensable to the Christianity characterized in the religious *Reden*. His philosophy was always for the sake of his theology of which it was only a broader form. God and the world, he says, are different expressions for the same worth, each unthinkable without the other. God is the essence of the world and not a will over it; hence there is no difference between His will and knowledge, between the possible and actual, between ability to do and performance. As providence is the law of nature, there can be no miracles, no origin of the world, no physical answers to prayer. Personal life is not the essence of the soul, but its phenomena, and the imperfection of the individual is but a part of the perfection of the whole. In short Spinozism is softened and idealized.

Corresponding to Kant's distinction between the sensory and the understanding, Schleiermacher distinguishes the organic from the intellectual function, the former as material and manifold, the latter as unifying, and each making and needing the other. All experience impels us to God, whom nevertheless we cannot know; yet he does not infer God to be unknowable, like Kant's thing *per se*, from the antithesis of our faculties, but because the nature of the ideas given in experience does not correspond to the God-idea which latter he thus illogically Zeller thinks presupposes. Our will he says fluctuates, but we vainly seek for the ground of will; while for Kant it is the will which first opens the intellectual world. The deepest problem—the relation between will and being—is found in personality, which is the appearance of the infinite spirit, and is the compendium of the universe. It must be developed to the fullest individuality between which and general laws there is no conflict. This noble romanticism, by which he strives to unite earnestness and scientific breadth and to rescue the abstract morals of Kant and Fichte from their subjectivity, makes our inmost nature the picture of the infinite, and personality the organ of knowing it. Here God and every moral principle is revealed, the knowledge of which we must work out into ever purer forms. Thus religion is feeling. Pure and perfect self-feeling, however, would be a knowledge of God without the world, which is impossible because the God idea can never be freed from antithesis. But conception also gives a notion of God, hence religion is not over philosophy, and while the former should be universal, reason and criticism are also allowed free scope.

Over against an absolute power and causality no feeling but that of dependence, or of being determined, is possible. This form of feeling is originally given with personality, before all self-activity. We dare not say that we know its source, as substance or otherwise, hence there is no God-idea, save the vague *whence* of the feeling of absolute dependence, and all attempts to personify God are gratuitous. Religion originates naturally in us, but we must actively develop it, and it remains ever imperfect; hence there are always parts of sin as well as of grace in us. The former preceding the latter, as the life of sense precedes the life of mind. Because as feeling, it is the most individual, religious life most needs enlargement in companionship. It is aroused by intercourse and needs community. This is the basis of the church. Every experience or representation of the individual life by word or act, which arouses others to responsively produce the same state in themselves, is revelation. This is best seen in the expressive individuality of a relatively perfect type, and others are aroused to discipleship.

So far Zeller essentially agrees with his teacher; but when the latter proceeds to make Jesus typically perfect—(*urbildlich*) as a historical person by virtue of his special religion or God-consciousness, Zeller objects that Schleiermacher failed to prove the latter, and that Jesus in no authentic passage claims typical perfection. Jesus may be a perfect man, but he is not thus proven a God-man, and as such neither his person nor the dogmatic deduction from it are natural. With surprising critical freedom Schleiermacher, in the matured form of his system, urges that all Christ's acts and words merely reveal his personality and so far as this creates or wakens my religious life, or in other words makes grace outweigh sin in me, he may be called my redeemer. There is absolutely no substitute or proxy bearing of the penalty of sin. The church visible and invisible, or actual and ideal or typical simply aids men to reproduce the image of Christ, as a norm in their own lives. Thus in explaining the creative beginning of moral life in Jesus and the church according to profound and more or less intelligible psychologic laws, in exploring the essence of religion, and in transforming its traditions to the spirit of our times, in giving even theology a new ground in modern consciousness, and in deriving all from self-consciousness. Schleiermacher's work is incomparable and imperishable. But when, as dogmatist, he treats the gospels, and even John before all, as historical and labors with such painful ingenuity to pour his new wine into the old vessels which Strauss and Baur were so soon and so easily to shatter, he was not only inconsistent with the freedom of his own earlier position, but brought long discredit upon his religious philosophy, the most profound and quickening in modern thought. Zeller's attitude to Schleiermacher is thus somewhat analagous to that of J. S. Mill to Comte. While reproducing and developing the spirit of his earlier best period with ripened and condensed vigor, he rejects the tortuous scholasticism of his dogmatic and exegetical [according to Darner his best and most matured] systematizing, as worthless. Religion is well called a feeling, but to describe its content as one of absolute dependence is inadequate or at least misleading. It is as well the consciousness of absolute freedom in a pregnant Hegelian sense. No matter how philosophic the conception of fatalism may be made, it must ever be prejudicial to moral accountability.

In discussing the teleological and mechanical explanation of nature, which is perhaps the most fundamental question of religious philosophy Zeller urges that it is equally senseless and tasteless to conceive animals as machines, the world as a huge time-piece, the mind as a body, attraction as caused by hooked atoms or to banish all notion of final causes as barren vestals on the one hand, and to explain trifles teleologically on the other. Neither can he agree with Plato and Aristotle that nature is to be explained in part mechanically and in part teleologically, nor with Leibnitz that the world as a whole is teleological and single phenomena mechanical. While the latter satisfies science it grants too much to metaphysics. All possible worlds had no struggle for existence in Gods mind, but the world as it is is the only possible form of its revelation, and is hence necessary. In a perfect nature the divine will and ability coincide with action. The world has no beginning or end in time. It was never without life and reason in some form. Because necessity is perfect and absolute it must be best conceived as imminently teleological, and the antithesis between the two applies only to its elements and not to the world as a whole. The need of the latter is only felt after man's acts have become plan-full, and if granted would require an infinite series of reasons, which at bottom would be, like the conclusions from logical premises, rather more mechanical than otherwise. Before matter, as space filling, moving, etc., can become an adequate logical cause of all things it must be conceived in a radically new way, while teleology is at most only one heuristic presupposition, and not a scientifically-grounded constitutive principle.

The development of monotheism Zeller considers as the most important of moral-theoretical problems, and among the Greeks the most gifted of all races, it is especially suggestive. The poesy of Homer and Hesiod depicts Zeus, the God-king, as subject to fate, surrounded by a turbulent and tricky aristocracy of deities, and although the protector of rights, as yet possessing no very moral character. His rule is far milder than that in the shadowy old dispensation of the Euminides. The poets were the first theologians, and it was they who reformed the crudities of the the early faith. Philosophy did not grow up, as since the christian era, in the service of theology. Xenophanes contributed the first monotheistic conception in describing men and gods as having one origin, and the Infinite One as being all eye, ear, thought, etc. From his keen irony *e. g.* in saying the Thracian gods have blue eyes and red hair, and that horses and oxen could they think and speak, would have quadruped gods like themselves, anthropomorphic polytheism never entirely recovered. The teachings of the sophists, which pervaded all ranks of society,—that we cannot tell whether the gods exist or not, that religion was the invention of shrewd legislators who sought in an appeal to fear the strongest sanction for their laws, or that the Gods represent those natural objects found to be the most useful, was followed by Socrates' conception of a unitary plan in nature and an all wise and good principle over-ruling all things. Yet he was by no means hostile to the popular religion, but believed in many Gods, who do all for the good of man, who must submit to and obey them, but also in a world-forming reason over all. The Eleatics believed in one only God, not in human form; the Cynics ridiculed the popular faith; the Skeptics declared it not proven, the Epicurians thought chance and necessity ruled the universe and that the gods led a life of placid repose, far off between the worlds and were worthy of unselfish veneration. Over Plato's eternal, changeless, ideal world, the Good rules supreme. It is the ground of all thought and being, giving to things reality and to thought truth. It is essential deity, towards which we strive in every act and thought, yet hard to know. It created and rules the world, is approached by purity of life, is not jealous of human happiness, is beyond feeling pleasure or pain in human acts. To this conception christian theology is immensely indebted. Yet Plato does not give up the idea of other visible gods. Stars, like the world, are incorporate deities. Men must be trained by mythic lies to later abandon figurate and poetic for true thinking. Aristotle reasserts most of the same notions but adds that God must be a personal, active, first moving cause, etc., his providence imminent. The Aristotelean conception requires polytheism only for political ends. The Stoic pantheism which held that creative fire, reason and law could all be worshipped, also granted that myths were indispensable allegoric representation of eternal elements. But the reaction of skepticism which, from its extreme distrust of reason, came to long for revelation and which, even among the Jews after the Babylonian exile, admitted the doctrine of angels and devils to gratify man's polytheistic cravings, led, among the Greeks, to the notion of demons, which were only the old deities of polytheism, as the servants and tools of the supreme being. The commingling of races, led to the conception of the later Stoics, that all men are children of the same father, to the belief in the unity of God, and to dissatisfaction with any merely national god or messiah. The last stand of polytheism was made in the new platonic philosophy in its long but ineffective struggle with christianity, which, refuting its central conception of a descending series of beings emanating from the one perfect light, which was at last extinguished in inert matter, adopted it as a form of speculation. Thus, Zeller argues, Greek philosophy prepared the way, though somewhat esoterically at first, or Christianity, and supplied the elements for its subsequent rational development to an extent hitherto unsuspected.

Pythagoras, after premising that the stronger the impression made by any person or event the greater will be the mythopœic reaction, he infers that the sage of Krotona must have been a many sided, earnest and sagacious ethical reformer. He came from his native Lanos to southern Italy in a time peculiarly fitted for his work. The central doctrine of the society of which he became the centre was that of future rewards and punishments and the transmigration of souls, or that moral purification was the highest end of life. This and his asceticism were perhaps learned in the orphic mysteries. He taught music—or the art of the muses—and gymnastics, and that all things might be expressed in numerical relations. His followers refused to eat the heart of animals because it was the seat of life, and were buried in linen garments that the suffering of wool-bearing animals might be mitigated. They held their goods in common, made fidelity the chief virtue, and taught that the best should rule. Such are probably the facts. Within four centuries after the master's death his followers described him as a prophet, whose head was constantly surrounded by a nimbus, who called up storms, healed the insane, arrested plagues, called down an eagle from the sky, ordered a bear to cease eating flesh and was obeyed, was seen at two distinct places at the same time, was called by name by a river god, remembered his preceding life in which he was the son of Hermes, as in this of Apollo, heard and taught the harmony of the spheres, had made a visit to Hades, etc. Nearly all distinguished men in Egypt and the east, it was said, had been his teachers. The older his school became the more his young disciples were able to tell of him. He left no writings, but in the first century B. C., many ascribed their writings to him partly as a compliment and partly to win consideration for them, till several scores of volumes now bear his name.

The germ of the Roman religion he finds in the Latin-Sabine veneration of invisible spiritual beings in nature. The solitude of woods, the gurgling of springs, the crackling of flames, the gloom of forests, the phenomena of the sky, of growth of the seasons, suggested to the old Romans three classes of natural forces, heavenly, terrestial, and subterranean, which were poetically personified as gods instead of scientifically interpreted. The transiton from a fanciful conception, to a matured ethical religion can be nowhere so fully studied as among the Romans, whose fundamental characteristic was awe of unknown forces, and constraint before supernatural influences. Hence their reverence for tradition their extreme care not to offend the gods by inauspicious chance words, by the neglect of the innumerable formalities which hallowed nearly every act of life. For centuries at first, like the Germanic races, the Romans had no or few images of the gods but later there were throngs of protective deities *e. g.* one for gates, another for hinges, one for doors another for thresholds, or again one for the cry of a new born babe, another for the father's acknowledgement of it, a goddess of the cradle, another who presided over the ceremony of naming, another was protectress from witches. There was one each for the child's food and drink, one which brought it from the cradle to the bed. Sacrifice was made to appropriate deities respectively that the boy's bones might grow rightly, when he first stood, walked, went to and came from school, to others that he might reckon, sing, be strong in body and in mind, etc. In the third and fourth century B. C., the influence of the religions of the north and south, especially that of Greece, began to be felt. First the mythology and rites, then the literature and later the Greek philosophy radically changed the popular faith and at last prepared the way for an easy transition to christianity. First the shallow Euhemerus taught that the gods were ancestors and Jove was the head of an old regent house. His doctrines were long influential. The epicurean deist Lucretius described the world as set free from the heavy oppression of superstition by philosophy. The Gods were far off and cared not for men. They could have no sex or age, the story of Iphigenia was an unmitigated horror. Scaevola declared that the religion of the poets was childish and often immoral and that of the philosopher abstruse and powerless, and held that religion was chiefly an art of the statesman, who must and ought to use it for political ends. That the *pontifex maximus* could thus hold dogma as nothing beside religious cultus without exciting antagonism is significant. Varro, the authority for most modern knowledge of the religion of ancient Rome, declared God to be the universe, especially the soul and reason. The public religion should be allegorized philosophy rather than the myths of the poets. Seneca's conception of a world-ruling wisdom, beneficent goodness, pious disposition, his description of deity, near, about, in us, was the highest form of Stoicism, in which it most nearly coincided with Christianity. Epictetus and especially M. Aurelius, to whom Zeller devotes a laborious essay, were far less emancipated from the popular faith. The former believed in Demeter and Persephone because men enjoyed their fruits, and because they restrained from wrong, and apparently never reflected that there is no error which may not do good at times, while the latter, too practical for the Stoic allegorization of myths, believed not only in dreams and oracles, but apparently in many foreign rites himself, and excused many other superstitions because they satisfied man's religious needs. Cicero held that faith in deity was deeply implanted in all men and was taught by the beauty and wisdom of the world, and that a pure heart was the best worship, and that whether or not the being of the gods could be scientifically proved, the natural religion must be strenously upheld as the chief bond of human society. After the republic the split between the doctrines of philosophy and the old Roman faith grew wider till the ancient gods lost their distinct individuality in the popular consciousness and the oriental monotheism of a denationalized Christianity readily absorbed all the purer and better elements of moral and religious culture into itself.

Nearly half of the first volume of the essays is devoted to a critical digest of the Tübingen school of theology of which Zeller is by far the ablest and most philosophical and perhaps its most moderate living representative. The middle, half-orthodox party, which rested upon Hegel and Schleiermacher's attempt to reconcile reason and faith, and which never had any logical basis, was broken up by Strauss, Baur and Feuerbach and all its ambitious and domineering or weak and dependent members betook themselves to the confessional hyper-orthodoxy which was then favored by the reactionary German courts, and church and state fell largely under the leadership of dogmatic fanatics or impatient hierarchs. Though now a tidal wave of reaction has strongly set in, the desolation thus wrought in the head and heart may still be seen in the fact that, while other sciences have progressed, theology has been stationary or retrogressive during the last half century. On the one hand are the free religionists in Germany, shallow, tasteless, unscholarly, without thoroughness or method, negative and eminently unprogressive, and on the other ultra-orthodoxy of the Hengstenberg type ever elaborating its uncritical gospel harmonies or an exegesis of the patristic type which can put any meaning into or out of the scripture text, and well content with working out practical unionistic platforms for evangelical co-operation between trivially diverging sects. Both are alike unsusceptible, he says, to the great pressing needs of scientific theology, viz., the explanation of religion itself from its psychologic and of Christianity from its historical grounds. The latter problem is by no means finally solved by the Tübingen school. Baur, its coryphaeus, held that the last result of the criticism of the New Testament and other early Christian writers should and would be a noble and at the same time historical picture of Jesus himself. This, however, so far from being given by the negative residual methods of Strauss, could only be reached after the bias of each evangelist and apochryphists, the authenticity of every text, as well as its historical validity, and every personal, dogmatic or philosophical party influence of the age should have been weighed and tested. It was to this, in some sense preliminary work, which Strauss, by destroying the foundation of dogmatic supernaturalism, made possible, that Baur mainly devoted himself; and the goal which inspired him, but which he did not attain, must be striven for and reached by his method if Christian theology is to maintain a respectable position in the modern intellectual world. Man's desire for happiness is oppressed by a sense of his finitude, but the true religious consciousness reveals a higher and compensating happiness attained by the culture of purity of moral disposition. Man's elevation through the religious consciousness above the finitude of his nature, expressed as poverty of spirit, humility, simplicity, unselfishness, and the inwardness and absoluteness of religious life characterized by the doctrine of the fatherhood of God, something like this Baur thought would be ultimately found to be the fundamental idea of Jesus, conceived with intense realistic ethical genius and made a pressing and practical question by being boldly and sagaciously interpreted as the bottom meaning of a coming Mesianic reconstruction of the Jewish state. In his earlier Hegelian period Baur regarded Christianity as mainly a philosophical, but later as a purely moral problem. The incomparable influence which Jesus started in history consisted not so much in any novelty of his conceptions,—these are now traced to earlier sources; but in the nobility of his character; the force and purity of his personality were so great that a new moral and religious type of life was inevitable. He *was* the Messias in his own inspiring sense and not merely claimed to be. This, as every such conception must now be, in the absence of reliable or detailed historical information respecting Jesus, is as yet too general and vague, and must be, on the one hand elaborated by a sound and vigorous ethical philosophy into a wealth of needed moral power too long unutilized, (somewhat as Pfleidener has since attempted, although the ethical genius of a Fichte is more adequate to such a work than that of Baur,) and on the other it must be verified and corrected by a deeper and stricter critico-historical study than even that of the Tübingen school has made.

One of Zeller's best essays is devoted to a characterization of his teacher, F. C. Baur, who, in his uneventful home-staying life, his slowly ripening nature, his amazing industry and perseverance, in philosophic, critical tact and vigor, in the growing importance and initiative power of his work, is aptly compared with Kant. His temperate mind could hold an important question open for years, sifting and weighing evidence with piety to every suggestion of fact, and so honest and *anima candida*, so without hyper-self-consciousness that he seemed like the noblest of the old reformers, while his moral sensitiveness was so acute that he was more grieved by lack of thoroughness or truthfulness in the work of his pupils than by the bitterest attacks of his opponents. He lived in and for his work, but could always preach edifyingly to the *Gemüth*, and his nature was profoundly religious and pastoral. His school, which has revolutionized religious opinions throughout Germany, Holland, Switzerland and even in protestant France, and has found many points of access even to English and American thought, is unlike the liberalism of deists, encyclopedists, etc., Schiller, Strauss and Feuerbach, in that it was founded by professional theologians and by men of deep personal piety. It simply drew conclusions which hovered in the intellectual air and which every one who thought logically must infer. It showed the time its own images and in urging that the New Testament was not pure history and not supernatural it only applied the critical methods which had almost revolutionized our knowledge of antiquity and its literature. Every one has smiled over the forcing, torturing and tasteless methods of the old German rationalism which explained away the miracle of Cana as a wedding jest, the fiery tongues of pentecost as electricity, the resurrection of Jesus as recovering from a trance, and declared that Paul at his conversion was blinded by lightning and was cured by the natural effect of the shock of an old man's hands, that the fetters were shaken from the hands of Paul and Silas in prison by an earthquake, that Jesus, though seeming to walk, on the water, really walked at its edge on the shore, etc.; in short, that oriental imagery and the reference of mediate natural processes immediately to God, which, though the exorcising supernatural, makes scripture none the less credible, but in an altered sense, even this had its effect upon the then current method of orthodox interpretation because it was no less tortuous and tasteless, as is perhaps best seen in the church history of Neander, who, without abandoning a single miracle or wavering on the doctrine of inspiration, which makes all Bible criticism impossible, yet loves to break off the points of the strongest miracles and is constantly conceding to the rationalistic methods, and capitulating to the *Zeit-Geist*, Bruno Baur, who has since declared the Tübingen school too conservative and apologetic, and been removed from his professorship, and who deduced Messiahship, resurrection and other evangelical *motives* from abstract dialectic principles ignoring or denying the existence of an historic Jesus. Marheinike, who made Bible texts into many-sided scholastic formulae, and Göschel, who all but identified philosophy and scripture, were alike unable to see the necessity or accept the results of such minute and painstaking researches as those of Baur.

First of all it must be borne in mind that the sense of literary property during the early Christian centuries was as undeveloped as any socialist could desire. Plato and Xenophanes put their sentences into the mouth of Socrates, [perhaps somewhat as a modern theologian states the *true Bible doctrine*, although in quite other than scriptual terms]. To present or develop the views of another, to win attention, to produce immediate effect, to seek shelter from criticism behind a great name, personal modesty, piety to a beloved teacher to whom now a days a volume would be dedicated—all these motives of apochryphal fabrication were so common that a moralist must be as *naive* and devoid of historic sense to raise the scruples of a modern conscience here, as to apply the laws against stealing in a modern statute book to the constitution of Sparta. The well proven cases of pseudonymous authorship in ancient times, many of which Zeller instances are, extremely numerous. Baur's conclusion that the gospels and the greater number of the epistles are unauthentic, of later origin and mainly records of violent partisan controversies which rent the earlier Christian party from its beginning, opens the most interesting and classic of all ancient literature to the use scholars, elevates of and frees the intellectual life of the age, to a degree to which only the work of modern science can be in any degree compared. It was his special endeavor to discover the bias or tendencies of the early Christian writers. In an age when men believed what pleased, interested, or edified them, whether that Homer argued for the Jewish sabbath, that Orpheus sang of Abraham, Moses and the ten commandments, or that an old or hardened heathen was converted by a relic stealthily laid under his pillow by night, in chiliasm, or that new records of the life of Jesus written by apostles were suddenly discovered at opportune polemic moments, and when credentials and criticism were all but unknown, the chief task of the historian is to seek and define the tides of party feeling and prejudices, the currents of men's wishes, ambitions and hopes, and occasionally political relations and the ground traits of individual character. These with traditions and sagas as material for a mythopoeic fantasy in a most agitated age of persecution and millenial expectations must be *controlled* before objective history can be reconstructed.

It was hard for the personal disciples of Jesus and the Ebionite party gathered about them to uphold his tenets against the dominant Pharasaic sect after their leader had been executed as a seditious agitator, but it was still harder for them to see Paul, who had never known Jesus personally, so successfully propagating his teachings, as not only independent of, but actually irreconcilable with Judaism, among Gentile races and even declaring that by it Jews were freed from their own laws. The conservative wing of the early Christian party, which held that Jesus could be the Messiah of the Jews only, and that the mosaic rites and laws were still binding as a propædeutic of Christianity, regarded Paul as an interloper who really designed to use the large collections he was making ostensibly for the church at Rome to buy the gift of apostleship. He is again even described as a conjurer who represented himself inspired till Peter exposed him. To define and defend his universalistic view, viz., that Christianity simply set men in right relations to God, Paul composed the letters to the Galatians, the Corinthians and especially that to the promising and hitherto neutral church at Rome. Meanwhile hard pressed and perplexed by the vast discrepancy between the actual low-born Jesus and the splendors of the Messianic kingdom of popular and patriotic hopes, the disciples had come to expect that he would appear again—an event by no means unparalleled in Jewish story and inaugurate a new kingdom of indescribable magnificence. Nero, the anti-Christ too, it was rumored was not dead but had escaped and would come again with oriental armies, and new wars and persecutions would most severely test the fidelity of the faithful. In this condition of things the book of revelations was written by John as a manifesto before its decisive struggle after which the millenial new Jerusalem, with Jesus as king, would fill the earth. Thus read it is no longer a puzzling riddle-book, but most historical and authentic, in fact the only book in the New Testament written by a apostle-disciple. The old Israelite expectations are all to be fulfilled in the wonder world of the re-appearing Messiah. Those who claim to be apostles but are not, together with the hated doctrines of the Nicolaitanss—bitter allusions to Paul and his teachings—are to have no place in the new theocracy, with its walls of jasper, its streets of gold and its tree of life. Thus too the most phantasmagorial dream of Jewish patriotism is successfully used to save the forlorn hope of a leaderless and losing cause. The controversy between the Pauline and the Jewish Christians was long and bitter, and colors, if it did inspire, most of the books of the new testament. The twelve apostles are paralleled, by the seventy co-workers of Paul. The Petrine party elaborated the Samaritan, Judean, the Pauline, the Galilean activity of Jesus. Peter is even represented as the founder of the first heathen church at Antioch, and is made to go to Rome because Paul had been there. James repudiates Paul's doctrine of justification by faith, urges that even devils may believe, and represents Jesus, perhaps his brother, as an ascetic Essene with long hair, and as abstaining from flesh and wine. In a word, Paulinism, which dispensed with offerings and with circumcision, and denied that the only way to the new faith was through Judaism, stands for the freedom of wisdom and mature manhood while the Jewish Christians argued for a status and moral regimen of adolescence.

Meanwhile both parties were persecuted alike, both were represented in nearly every church, practical, administrative, unity became more essential as the church began its immense organization, while old passions and prejudices only faintly survived in a new generation. In the second century a conciliatory desire to save the effects of the work of both wings is manifest by accommodating, and often even transforming their destructive tenets. Thus Acts written in the second century and based perhaps on notesof Paul's traveling companion, and Luke, though both written with unmistakable Pauline drift are very conciliatory, Colossians ends with complimentary mention of a list of Petrine worthies, while like Ephesians its Paulinism is very tempered. The first epistle of Peter makes surprising concessions to Paulinism. On the one hand it was apparently granted that Paul was too intricate and speculative, and that faith alone was not enough for salvation and on the other it was necessarily acknowledged that the wall of partition, between Jews and Gentiles was broken down in fact, and the vast number of non-Jewish Christians were taken into fellowship. Thenceforth all traces of primitive discord were carefully scored away, and the energies of the church were free for the work of practical and dogmatic development and defense. As the church grew, all parties united to elevate the conception of the person of Christ still higher, a convenient point for dogmatic unity in zeal for which old discords might be forgotten, until at last even the Messiah idea with which it had become identified in the first century was not exalted enough for the head of a church that had its stronghold in the capital city of the world, and was destined to become universal, and of a hierarchy so rapidly growing in influence and self-conscious dignity. The son of David gradually became with the growing influence of the ultra-Pauline Gentile element and the Alexandrian gnostic-philosophy not merely the heavenly pneumatic man, the new Adam, but the pre-existent, creative Word. The gospel of John, (A. D. 170-180) which is not historical, but represents the maturest and best points of the work and teaching of Christianity up to the period of its composition, which quietly appropriates the serviceable elements of the dangerous heresies of gnosticism and montanism, and shows no trace of heirarchism in the church, marks the point where the history of primitive Christianity ends and that of catholicism begins. The charm of the Johannean image of Jesus, so pure, so exalted and almost femininely delicate, so harmonious that his inward peace was undisturbed by conflict and sorrow, and so free from all earthly limitations is unprecedented among all ideal personalities hitherto offered to human contemplation. The Johannean gospel not only reconstructed the previous Christian history from its new and tranquil stand-point, but represents the highest theological development of the first period of Christian history.

In an essay entitled Greek and Roman Prejudices against Christianity, Zeller shows that while the reign of Alexander and the Roman Empire had prepared the way for the outward spread of Christianity, the popular Stoic philosophy, which taught that all men were brothers with equal rights aud duties, and subject to the same moral law, which instead of faith made ethical temper the saving principle and divided mankind into two great classes, the fools and the wise, instead of the redeemed and the lost, and which longed for the" birth-day of eternity," an entrance into the "great eternal peace," prepared the mental soil for the reception of Christian doctrine. The popular heathen notion was that the Christians, if not all Syrian barbarians, were yet atheists, criminals, who perhaps cooked and ate children, prayed to a God with an ass' head, were the worst and most unpatriotic citizens, and in fact enemies of the human race, so that Nero found no difficulty in circulating the report that it was they who had fired Rome. Pliny thought their creed in itself a harmless superstition, but believed their stiff-neckedness in refusing to adore images of the Gods and the Emperor, and in violating the laws against making proselytes should be punished. The mild M. Aureleus persecuted them because he deemed the pertinacity of their creed,—so unlike his all sided toleration and uncritical eclecticism, dangerous to the dicipline of the state. Lucian said the sect was composed of pitiable and deluded disciples of an arch sophist. The platonic Celsus argued that Jesus had stolen and disturbed philosophic doctrines which he could not understand, was of dishonorable birth, and a conjurer. Greek joyousness and Roman pride had only contempt for a religion designed for the sorrowing, oppressed, weak and guilty. The Neoplatonists revered Jesus, but one inquired like Reimarus, why, if salvation was through him, he appeared so late, and urged that if Peter and Paul could disagree about fundamental tenets his doctrine must be very uncertain, another thought Jesus did too few miracles to be really a god and proved that Apollonius did far more, while Julian, believing it was impossible for all men to have the same religion, argued that all noble men and great deeds in the world had come from heathendom and forbade Christians to teach the ancient literature.

In the saga of Peter as Roman Bishop the ultra ebionite view of Paul, which described him under the name of Simon the gnostic Samaritan sorcerer, who, after he had been exposed by Peter in Palestine came to Rome, where, by his arts and by the aid of demons, he had won great honors and many followers, is the ground motive. Later when Romish canonists sought to derive the power of the popes directly through Peter it is said that the latter followed Simon to Rome. The Jewish legend dishonored Paul whom the catholic party would honor, hence he is now distinguished from his double and made to join Peter in opposing Simon, and both Paul and Peter it was said died in Rome. Later Peter alone is made the first Bishop of Rome and thus the greatest work of Paul's life is accredited to the hostile apostle of the circumcision.

Much importance is ascribed to Schwegler's work on Montanism and the Post-apostolic age wherein it is concluded that Christianity assumed at first to be nothing but a more complete form of essenic Judaism, and that the autonomy and universality which Paul attempted to give it, transformed and dejudaized it materially less than had been generally supposed.

In the Platonic republic Zeller sees not only a significant ideal and prophecy impossible of realization, despite Plato's unreserved belief in it, and not only a product of the time, when, after the Pelloponnesian war, thedangers of individualism, the greed of riches and party strife seemed to show that men could not be trusted with their own development, but especially a type of society which has been no less than a germ for the organization of the mediaeval church. Instead of the philosophers who were to rule absolutely in the Platonic republic, are the priests, instead of the warrior cast, the temporal powers, instead of community of goods which was an early Christian ideal voluntary poverty of goods or of spirit and the mendicant orders. Community of wives, which was recommended to restrain not to satisfy desire, is paralleled by celibacy that monks may live all for the church. Both the ideal and the actualized system rest upon ethical dualism and teach that suffering here will be compensated in a future life and both assert a divine leadership of the state. The republic, like the kingdom of God, is an institution for training men in virtue. The church on the other hand does not so absolutely subordinate the individual to the community and the spirit of universal fraternity is widely contrasted with the casts and the national exclusiveness of the republic. While Plato would class modern theology, so far as it does not coincide with philosophy as mythology, and would be able to find in modern universities no suitable philosophers for rulers, and would be incensed at the modern political romances, wherein private interest is satisfied instead of annihilated, he must nevertheless be counted as one of the most important predecessors of organized Christianity. Much space is devoted to show that this was not the result of mere analogy but was history and that Plato's conception, at first too spiritualistic to be popular, had passed into the general culture of the day.

This matured and moderated digest of the Tübingen school so briefly and imperfectly epitomized and digested above, records, we believe, the most important achievement of the historic critical method. It affords the general terms of a suggestive and edifying solution of the most intricate and also the most obscured of all historical problems,—a problem not of one sect and race or century, but of commingled nationalities of contending political and philosophical, religious, partisan and personal interests. The facts were so inaccessible and so metamorphosed in this long contest, that only the most patient and conscientious research coupled with amazing psychologic insight and tact was able to reconstruct them at last after ages of misconception, with so high a degree of verisimilitude that the most distinguished of Roman historians, whose essential impartiality cannot be denied, declared that several years ago no German scholar under forty-five had thoroughly studied the Tübingen writers without being in the main convinced by them. There will long be many to fear that moral restraints may be practically weakened if scripture is proven uninspired in the old sense, or if miracles are disallowed, just as the Emperor Julian feared that classical literature would be ignored and perhaps lost if faith in the ancient gods was destroyed. This is, without doubt, sometimes the case among the young and the undiscriminating. But on the other hand it is only thus now made possible for men of thorough modern culture and moral self-respect to call themselves Christians if they will, and to be so in mind and heart in a sense deeper and larger than many conventional churchmen comprehend, and even if they see fit and hopeful occasion, to urge friendly even though misconstrued aid in ameliorating the narrow severity in faith and life, and in sustaining and reforming church organization, as a right by no means invalidated by stricter modern definitions of the Christian name but rather new vested by the supreme sanction of a positive and adult moral understanding. Myth is a deeper and broader expression of humanity's common nature and needs than reason itself has yet attained. It is never the utterance of the mere individual, but is the *logos*, or over-soul of the half-unconscious moral instinct of a race or an age. It is never bound too closely to details of place or time. These only hinder or embarass its rare and strange moving and edifying power. In its noblest scriptural form,—Biblical in the classic sense wherever found—it comes most clearly and directly home to the *Gemüth*, takes men out of their own selfish personal lives, and raises, purifies and broadens their motives and feelings and purposes, as nothing else does. How to make it most effective for good is a problem which *homiletic* art has perhaps not yet finally solved.

The "Tübingen men" in Germany have grown now inactive and retrospective, and even Zeller is somewhat prolix and boastful in his recapitulations, and yet not only is their critical work incomplete, but its practical deductions, (the last consideration of a German savant) because left to be drawn in a negative, popular and superficial way, have been often sadly injurious instead of most helpful as they should be to the cause of religion and morality, and the German capital has grown perhaps more unconscious of the existence of religion and its institutions than any city in Christendom. Far from assenting to any ultra theories as *e. g.* that of Rothe—that the modern state more than the church expresses the essence of Christianity,—we cannot deny that the latter has grown far too consubstantial with our social, moral, intellectual and æsthetic life and development to be eradicated by any violence, or even to be intellectually distinguished and traced through all the long and subtle associations, by which it has become ingrained in our inmost psychic character. By being proven the oldest of all historic categories, and rooted in the earliest written records, instead of a supernatural graft upon an old and decaying trunk, it challenges the reverence of science itself as the most important problem of popular (Völker) psychology, by contributing to the experimental solution of which all known civilized races and ages have become in a noble philosophic sense organically united. As the modern musical scale, and the masterpieces composed in it are not endangered by the proof of its mathematical inaccuracy, its rude empirical origin, or by the suggestions of improvement, as the modern state is not lost to socialism by the demonstration that all values originated in the ten fingers of the working man, or that the rights of bequest and of absolute private ownership are, so to speak, recent habits, resting upon a series of accidents and misconceptions, so the Christian church is by no means essentially or permanently weakened by being compelled to relinquish its belief in miracles, inspiration and an incarnate deity for more historical conceptions of its origin. It is only another reformation that impends, as radical, possibly, to the more assumptive and unreasoning of modern Pharisees as was the new dispensation of Jesus itself, but only salutary to every true religious interest.

A brief notice of some of Zeller's less important essays will perhaps convey a better idea of the range and minuteness of his learning and of the acuteness of his critical power. In his defence of Xanthippe he reminds us that the young wife of an old man who could humorously boast of the advantage of ugliness like his over the classic Greek type of beauty, that the bridge of his nose was low, that with one of his prominent eyes he could look directly into the other, his mouth so large that he could save much time by eating faster than others, and his lips so thick and soft that he could give and receive the sweetest kisses, and whose ponderous body was the type of Silenus, might be excused for not being proud of the most monstrous among all the handsome Greeks for a husband. Moreover he would seek no office, lounged all day on the streets and in the public marts talking with tailor, shoemaker and even *heterai* about the dialectic conception of their profession, and although so poor that wife and husband had but one outer garment between them so that one must stay at home when the other was out, would sometimes stand all day in one spot lost in revery, ridiculed by boys and comedians, and at last come home, old and fat as he was, to practice a dancing lesson for which, perhaps, he had paid his last *heller*. Moreover the suspicion that he had married her as a discipline in patience would hardly have been delicate and flattering to a woman's nature. When she came sobbing with a child in her arms to see him for the last time in prison before the fatal draught of hemlock, and severely looking at her he ordered Krito to take her home, and when she had been removed screaming, he calmly began a philosophic discourse. Possibly Xanthippe threw dirty water upon him, attempted to tear off his garments in the market place, overthrew his table and trod upon a cake that had just been sent in, no one knows from whom, and perhaps Soccrates consoled himself that she never kicked or bit him, but more probably these are unsalted inventions of lively Greek gossips or chronicles to make the name of Socrates brighter by contrast. She was probably no worse than many a modern woman would have been with her provocation.

Very readable is his characterization of Alexander the Paphlagonian imposter and Peregrinus the enthusiast. The former was famous for his beauty, and lived in the time of Trajan. Planning to found a new oracle at Abonuteichas, he buried and caused to be found, brazen tablets announcing that Askulepios and his father Apollo were about to remove thither, caused it to be announced that he was the grandson of the former, and later appeared himself in purple with the sword of Perseus in a feigned exstacy and with artificial foam flowing from his mouth. Throwing aside all his garments he showed the assembled multitude a young serpent in a broken egg shell, and a few days later an immense artificial snake with a human face, with eyes and mouth worked by invisible hairs, which he declared had grown from the little one and was a new God, Elycon, and from whom he would receive divine messages. Sealed letters were sent, and if they could be opened and sealed without suspicion were returned with answers written beneath every question. He hired a *claque* in distant cities and finally in Rome who reported the most astonishing miracles—hidden treasures and thieves discovered, the sick healed and even the dead raised. Messengers were bribed, difficult questions generously referred to the priests of other oracles, until at last Rutilinus, a man of high standing in the Roman court, like another Zöllner, fell completely into his net, and he became the fashion in the imperial city to which he graciously offered his protection against pest, conflagration and earthquake. He became immensely rich and made it dangerous for rationalistic epicureans or for Christians to attempt to expose him, and died at the age of seventy with undiminished fame.

Peregrinus of Pazium gave his fortune to his townsmen and traveled in the east where he learned the "rare wisdom" of the Christians who, it was said made him a bishop. Later he appeared in Rome as a cynic, anathematizing all the world, and especially the Roman emperor Antonius Pius, who banished him from the city limits, beyond which he lived in a hut and attracted many young men by his philosophical discourses. He afterwards went to Greece, and when no one took further notice of him, announced at the end of the Olympic games that at the end of the next festival he would burn himself alive. When the time came he had an immense pile erected, made a long harangue to the curious crowd, enumerating all the privations and sufferings which he had borne in the service of philosophy, declared he would die, like Heracles, to teach men to dispise death. As, contrary to his hopes, no one interfered, but rather when a single voice cried "save thyself to the Greeks," the crowd vociferously exhorted him to courage and the speedy accomplishment of his purpose, after adjourning the act till another night "that the moon might also see it," clad in the Cynic uniform, casting a handful of incense into the flames and commending himself to the spirits of his ancestors, he walked tremblingly and pale into the flames and was seen no more.

In "A Strike in Rome" Zeller discusses the variously recorded story of the origin of the Roman festival quinguatrus. The pipers, it is said, vexed by certain restrictions of their prerogatives, withdrew to a man to Tibur and occasioned thereby great distress in Rome. There could be no festive sacrifices to the gods, no religious processions, no marriages, no burials. The senate in vain tried to induce the irate musicians to return, and only after they had been made drunk at a feast given in Tibur and brought home in wagons did they consent, if all their ancient rites were restored, to resume their duties as before. This is compared with the legend of the origin of festivals of *carmenta*. The Roman matrons of old had the right to ride in carriages, of which they were deprived by the senate. They all swore to bear no more children till the privilege was restored, which the senate hastened to do. Both these tales, the former of which has been hitherto undoubted, Zeller argues with great ingenuity are instances of the ætiological sagas so common among the Romans and utterly without historical foundation.

In characterizing Fichte as a politician we are told that he possessed the very rare combination of great scientific acumen and culture, with immense vigor and sensitiveness of moral character. It was the substance of his philosophy that the will of the individual created not only his own character, but his own world, and that individual action and development might be free and unhindered, was the ground motive of his life. The true state itself is only a three-fold compact of the sovereign people who can therefore never rebel. Its business is solely to protect men in their rights. To this end they must oversee all departments of work in every detail, and cause every one to be remunerated according to his services. This view has made him a favorite with the modern socialists. It alone controls intercourse with foreign states and its citizens should have only its money and never that of other countries. So long as the state is anything other than the spontaneous organization of the people, the latter are not free. An absolutely free people would need no state. The fate of the true culture of freedom rests with regenerated Germany. Her language which has been developed indigenously, without obscured etymologies, from a primitive kindred people and not adopted or borrowed, or adumbrated by change, like that of other European nations, makes true mental freedom possibly only for her people. His philosophy and his political theories, it is concluded, are both superceded by later and better views, but will yet long remain, even where most contradicted, very instructive and elevating.

In Wolff's expulsion from Halle, Zeller sees the pure epitome of a contest which is not yet ended. At the close of the thirty years' war Germany had grown barbarous, ignorant, schismatic, sensuous in taste and life to a degree which German patriotism now finds it hard to admit. Protestantism had fallen into the hands of men whose rule was scarcely less fruitless, formal and unprogressive than that of the Jesuits then dominant in the catholic church. It had no understanding of the religious needs of the people and had driven edification from the church, learning from the schools, and freedom and thoroughness from the universities and from literature. At this period pietism and philosophy first took their rise in Germany. Spencer, reacting against the dry and dead intellectualism of theology, urged at first a most salutary form of emotional and practical religious living, and argued the necessity of a definite and typical change of inner life which found wide acceptance and has founded the Lutheran church deep in the *Gemüth* and given it its peculiar freedom and independence of scientific reason. Wolff, whose methods affected mathematical form and certainty, who used the vernacular tongue in his thronged philosophical lecture room, who had argued that even an atheist might lead a moral life, and that if no divine reveletion had been made even reason would incline men to virtuous lives, was violently attacked by the pietists and obliged to enter into tedious and profitless disputations. He saw his students and followers and even his friends gradually alienated from him until at length the king of Prussia, induced by a plump lie of an enemy of Wolff, ordered him to leave his domains within forty-eight hours on penalty of being hung, and made it a crime to circulate or read his writings. This his pietistic colleagues declared was in answer to their prayers. He was recalled late but not until his vigor and his influence were forever impaired.

The relations of church and state in the past and present and the discipline, cultus, orders, property and influence on education of the former are discussed from an abstract, moral stand-point in a readable little volume, which space fails us to epitomize. (1). In another essay the trial of Galileo studied in part from original sources, is described, Schwegler, Waitz and Lessing as theologians are characterized, the relations of policy and justice, and of nationality and humanity are discussed, and the present condition and problems of German philosophy, and of the *theory of knowledge* (Erkentniss-theorie) are explained.

The latter has been the central question in German philosophy since Kant brought into flux the question of the origin and truth of our notions of things. Based upon special solutions of it, the great idealistic systems were wrought out. The cry " back to Kant," and the general abandonment of the foundations upon which Fichte, Schelling and Hegel built, which in many quarters has degenerated to an uncritical cultus of Kantain orthodoxy was at first matured by the conviction that, he alone had fairly examined and justly estimated the importance of the "theory of knowledge," while later the experimental psychology of the physiologists and the studies of Helmholtz have only more specially elaborated this theory and more critically answered Kant's problem. We must not infer from the study of Kant that experience can give *unordered* matter, or that all form is innate, still less can we agree with him that because we apprehend things by means of subjective forms we must necessarily be ignorant of things as they are in themselves. There is another case. Perhaps the forms are adapted by nature to give us the *right* view of things. Subjective and objective belong to one nature. True if we isolate one phenomenon we cannot distinguish its elements, but every new observation applies the method of difference. We prefer to say that with the increasing compass and accuracy of our knowledge, it approaches practically—though not theoretically in the sense of Fichte and Hegel—to absoluteness. It necessarily grows certain as it grows wide. It is reflection which sifts out the *a priori* elements from experience and thus brings knowledge of things. Hence logic is grounded on the theory of knowledge, which must in turn be completed by it. Number, time and space Zeller makes the most general forms of connecting objects. Properties are causal ideas which are not innate in the old historic sense—so intimately connected with the doctrine of pre-existence—but they are hypotheses to explain the unifying impulse of the mind. Space, however, unlike the other two which are objectively real, at the basis of being and change, may be only the general way in which things impress us, or a general form of reaction of our organism in its habit of connecting sensations. Different hypotheses of the external cause of sensation may supersede it; or again tri-dimensional space may be only a special case of another relation, embracing other cases also.

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(1). Staat ú Kirche; Vorlesungen an der Universität zu Berlin, Gehalten von Edward Zeller, 1873, p. 250.

Here at last we glimpse a limit to Zeller's remarkably wide critical horizon which is particularly manifest throughout his courses of psychological lectures. The hypothesis of a fourth dimension of space, in no way destroys the validity of the old geometrical three. Mathematical physics has elaborated equations containing functions which might be true in a space of x dimensions and forthwith metaphysical psychologists, reasserting old idealistic traditions, or perhaps too easily bullied by scientific authorities, stultify science by talking of an absolutely spaceless universe, and a non-extended matter. To Zeller this is only a logical possibility which must not be forgotten. Like many of the older German professors of philosophy he does not deem it all too unworthy the dignity of his department to interest a curious class by recounting some of the more striking results and methods of the physiological study of sensation, but too often only to disparage their philosophical importance and to limit to the narrowest the impressions deduced from them in detail while roundly acknowledging the general importance of such investigations for the theory of knowledge. The fundamental importance and the immense scope of these, centering as they do about the transforming psychological conception of reflex action modified by specific nervous functions and inhibition, Zeller fails adequately to appreciate. We will pause here only to observe that the whole drift of German physiology is now strongly and almost without exception against the possibility of such materialistic deductions as Zeller fears therefrom.

Philosophical truths to Zeller are not coins stamped and weighed to pass unchanged from hand to hand, but historical products deeply rooted in personal, national and religious character. As such they must first be approached and studied if we would add our own individual thinking as a contribution however trivial to the thoughts of the race, instead of reviving old issues, resolving old problems and thrice slaying the slain. The history of philosophy is thus a labor-saving department of study most economic of mental effort, and prepares men for the problems of to-day. It should aim in the first place simply to present and not to criticise or estimate its subject matter. It should teach us how our consciousness became what it is. It should show that all practical sciences or institutions of human life and society pre-suppose a theoretical foundation which is deep and broad, in proportion as they are high or important. Not only do the roots of all things go back to philosophy but it is an unnatural condition of things if philosophy is suspected or degraded. As Cavour said the state should be occasionally led back to first principles, even by revolution if need be, so it is we not to allow men entirely to forget how law and every political and social institution were at first and still are at bottom, only devices to establish simple morality as an individual habit, and between man and man, and that all religions are but formulations of man's relations to the universe as a whole. Moreover the special branches of knowledge are able to act and react fruitfully upon each other only as it is seen how organically they are connected. The effect of science upon philosophy may be in some sense compared to its effect upon poetry. Since it became impossible to believe longer in myths, poetry, instead of being crippled or suffering any limitation of her domain as many predicted, has found new sources of inspiration deeper and stronger than ever before while even historic myths exert undiminished magic charm over the imagination of men. So likewise the metaphysical myths, Platonic ideas and ideals, innate intuitions, an absolute ego, a dialectic, world-developing reason, a universal will, and scores more, are no less quickening now than before while the observational and more exact experimental study of the psychic powers are opening up a radically new conception of the human soul, reason and conscience. With this is suggested, at least to those where supreme passion of life it is to conceive it however faintly, the possibility again of one organized intellectual world manifestly monistic, without unscientific, hyper-logical guesswork, in which idealism and realism, instead of being absolute even in their opposition are simply two cardinal points of direction of which philosophic thought must not lose sight.

In his somewhat popular history of German philosophy since Leibnitz [1] written as the thirteenth volume of the History of Science in Germany under the auspices of the Saxon commission, somewhat monographic, and mainly devoted to the seven great names from Leibnitz to Schopenhauer both inclusive and not to be compared with Kuno Fischer's exhaustive work in the history of modern philosophy, Zeller urges that the reformation made Germany introspective. The deepest roots of her power in the world's history he finds in her philosophy and more especially in her idealism at once its weakness and its strength. Germany will be false to all her traditions if she forgets the power of subjective reflection. Her philososhy was developed in a period of peace unbroken save by the inspiring war of liberation, and even now with all her political military and material successes, her growing love of money, and devotion to business must be guarded with pious patriotic care as yet full of saving and guiding power.

Zeller's great life-work is of course his history of Greek philosophy, the first part of the first edition of which was printed more than a quarter of a century ago and which has now reached a fourth edition. It is by far the best work on the subject. His characterization of the pre-Socratic philosophy, though as unlike the speculative histories of Hegel or Schwegler as possible, is a masterpiece of constructive criticism. The laborious minuteness with which every trace of suggestion is followed up, the compass of his method which requires familiarity with every phase of contempory Greek life and history, the conscientious care to avoid all false idealizations and to hold every personal preference or prejudice in perfect poise and his constant verification by quotations have all combined to make his readers conceive of Greek thought as perhaps less pure and perfect and less transcendently wonderful than we were wont, but have invested the theme with a nearer and far more sympathetic human interest than ever before. It is of course impossible in our limits to enter into any detailed review of this work, but this rough sketch of its author's varied intellectual labors will not have been written in vain if it shall induce the reader to take this work seriously in hand for himself.

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[1] Geschichte der Deutschen Philosophie seit Leibnitz von E. Zeller, 1873, pp. 917.

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A SKETCH OF THE HISTORY OF PSYCHOLOGY AMONG THE GREEKS.

BY CHARLES A. STRONG.

The following paper is an abstract of six lectures delivered at Clark University in the autumn of 1890 on the history of psychology among the Greeks from the earliest times down to Aristotle.[1] The psychologists of this period are the philosophers, and their psychological doctrines are for the most part so intimately bound up with their philosophy that a sketch of the former necessarily involves some mention of the latter. We must therefore devote a few words to the metaphysics of each philosopher before taking up his psychology. But the psychological theories of the Greek philosophers stand in the closest relation to the animistic beliefs that prevailed among the early Greeks; and our sketch would be unintelligible without a preliminary account of these.

I.

Long before scientific psychology begins, there exists an ancient popular psychology, which embodies the earliest naïve notions of uncivilized men about the soul and its activities. These notions are found among barbarous and semibarbarous tribes in all parts of the world. Their universal diffusion is better authenticated than that of religion itself. Linguistic research proves that they were entertained by our Aryan ancestors, and the Homeric poems furnish the amplest evidence that they were shared by the early Greeks.

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[1] The chief authorities on the psychology of the Greeks are the great work of Zeller, Die Philosophie der Griechen, of which I have used the last edition; and Prof. Siebeck's Geschichte der Psychologie, Gotha, 1880-84, which is completed as far as Thomas Aquinas. On the animistic conceptions of the Homeric Greeks, see Erwin Rohde, Psyche: Seelencult und Unsterblichkeitsglaube der Griechen, Freiburg i. B., 1890, pp. 1-11. On the relation between Greek psychology and animism, see Julius Lippert, Die Religionen der europäischen Kulturvölker, Berlin, 1881, Einleitung and pp. 250-275.

The commonest notion of the soul, according to Tylor,[1] is that of a thin, unsubstantial human image, in its nature a sort of vapor, film, or shadow; the cause of life and thought in the individual it animates; . . . capable of leaving the body far behind, to flash swiftly from place to place; mostly impalpable and invisible, yet appearing to men waking or asleep as a phantasm . . . ; continuing to exist and to appear to men after the death of the body . . . . . ." The soul is thus that whose indwelling in a man causes him to be alive, and whose departure causes him to die. Its existence is assumed in the first instance for the purpose of explaining the difference between life and death. The movement and activity of living beings must, it is felt, be due to some interior cause, and this cause is called the soul. The abrupt transition from life to the stillness of death is explained as due to the departure of the soul.

That this is the true origin of the notion of the soul, we have evidence in certain further beliefs that are well-nigh universal among uncivilized peoples. In the first place, the soul is represented as analogous to, if not identical with, the breath: hence the Hebrew words *nephesh*, *ruach*; the Greek *psyche*, from *psychein* to freshen with the breath, and *pneuma*, from *pnein* to blow; the Latin *animus*, *anima*, connected with the Greek *anemos*, wind, and *spiritus*, from *spirare* to breathe. Now the commonest observations of the difference between life and death would naturally lead men to connect the soul both with the breath and with the blood. For, in the first place, loss of blood means loss of vital force, and if too much blood be lost death is the consequence. In the second place, men breathe as long as they are alive, and cease to breathe when they die. Furthermore, the savage has no clear conception of the function of the lungs, but supposes that in some way the inspired air gets into the blood and is carried by it all over the body. It is thus a pretty consistent theory which identifies the soul with the breath, and finds its special seat in the blood.

Such, then, were the conceptions of the Homeric Greeks regarding the soul and its relations to the body. In what sense they conceive the soul to be the cause of life and movement appears from their views of its condition after death.

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[1] Primitive Culture, I., p. 429.

When death overtakes a Homeric warrior, his soul escapes through his mouth, or through a gaping wound, and hurries to the house of Hades. When separated from the body it is called *eidolon* or image. These images are thin and unsubstantial as smoke or shadow; being "as the air invulnerable," they elude the grasp of the living. Their life in the lower world is a pale, disconsolate one; indeed, they can hardly be said to live at all, for they no longer possess consciousness and volition; the truth is that "they do not live, but only exist." There is, however, one means by which they can temporarily recover life and consciousness, and that is by partaking of blood. The soul cannot therefore, in the conception of the Homeric Greeks, be said to be the independent possessor of life and consciousness. Only so long as the soul remains connected with the body — only so long as the soul retains its union with the blood — does mental activity continue. It follows that sensation, thought, and volition are functions of the living being which soul and body constitute, not of the soul alone.

While the blood in general is conceived to be the seat of the soul, the mental faculties are assigned a special seat in the breast. Thus the word for midriff or diaphragm is the common expression for mind; for the main reservoir of the blood is in the breast, and the midriff is put by metonymy for this entire region. Similarly, the various words for heart are used to denote the subject of the states of feeling. In general, the attention of the Homeric Greek is turned more especially towards the robuster states of feeling; which explains why the psychical activities, including even perception and thought, are supposed to have their seat in the breast, and not at all in the head or the brain.

II.

Down to the time of the Sophists, the Greek philosophers are mainly occupied with cosmological problems. When the Ionics declare that all things consist of water, air, or fire, they do not mean by water, air, and fire just what we mean by these words. The conception of matter as matter, that is, as lifeless, passive, inert substance, is a late scientific product. Nor must we imagine that the problem they are endeavoring to solve is a purely physical and not rather a biological one. They are not leaving living beings out of account, and seeking simply to explain the mutations of matter; on the contrary, they regard all matter as alive, and if they select water, or air, or fire, as their fundamental principle, it is because they think they see in this form of matter rather than in any other the essential basis of life.

The early Greeks, as we have seen, consider the soul to be closely connected with the breath, and to have its seat in the blood. Now there is a difference between living and dead blood; when the blood of a dying man flows out into the air, it grows cold, coagulates, and dries. Thus, at the same time that the soul disappears, the warmth and moisture of the blood disappear. It is furthermore a striking fact that the breath also is both warm and moist, and that it leaves a palpable residuum of warm moisture behind it. What more natural, then, than to take this warm moisture which is the common element in both blood and breath, and identify it with the soul?

This is what *Thales* does when he declares that the first principle of things is water. Water, he says, is the substance of which all things consist. In men and animals, it is the warm moisture of the blood upon which life and movement depend. Since this is so, the life and movement of external nature are to be accounted for by the same analogy; they must be due to the fact that all things are at bottom forms of water. Hence his statements that "the whole world is alive and full of gods," that is, of souls; and that even the magnet has a soul, since it is able to produce motion.

Aristotle conjectures that Thales may have been led to his theory by the observation that the food of all animals is moist, and that they all originate from moist seed. However this may be, we know that *Hippo*, a contemporary of Thales, who agreed with him in identifying the soul with water, or rather with moisture, strongly combatted the traditional view that the soul is in the blood, and maintained that it is in the seed. He seems to have supposed that the seed is not only the starting-point from which the new individual is developed, but remains in the body throughout life and forms the nucleus in which the vital activities centre. We thus have two opposite theories of psychogenesis—the one that the soul is in the blood, and is therefore derived from the mother; the other that it is in the seed, and therefore derived from the father.

Anaximenes regarded air as the stuff of which the soul consists, and held that all things are formed out of air by condensation and rarefaction. This theory is little more than a philosophical re-editing of the popular view that in breathing the soul is nourished by the inspired air, which, it is argued, must therefore be of the same nature as the soul.

Diogenes of Apollonia agrees with Anaximenes in identifying the soul with air. The centre of life and thought is the heart; here the blood is formed, and here it is mingled with air, which it carries to every part of the body. "The life which pervades the entire body has its source in the blood, which is foamy and filled with air." He also points out that the seed is foamy and filled with air, "like the blood"; and does not forget to insist that in both cases an essential quality of this air is its warmth.

It is only an accentuation of this view when *Heraclitus*, the greatest of the Ionic philosophers, maintains that all things consist of fire. By fire he does not mean flame, but a dry warm vapor, which he conceives to be the essence of fire. Fire, in this sense, is the stuff of which the soul consists, and is present in the breath as its essential constituent. Heraclitus has by no means abandoned the popular notion which finds the principle of life in the blood and the breath; it is only a different constituent of these which strikes him as essential, namely the quality of warmth. On the one hand, he is deeply impressed with the phenomenon of animal heat; on the other, the observation of the subtle, penetrating, mobile, destructive character of fire outside the organism convinces him that this, of all other natural substances, is the principle of life and activity. All things consist of fire, but not all things manifest the familiar qualities of fire. For all things are in eternal flux; all things are continually changing their qualities. This happens because all things are continually undergoing either condensation or rarefaction. In their state of highest rarefaction they are what we call fire, and then it is that they manifest the qualities of soul; but the process of condensation now transforms them successively into air, water, and earth. The human organism is compounded out of the elements at the bottom and those at the top of the world-process, in such a way that the body consists of earth and the soul of fire. The mechanical bond which connects the individual soul with the diffused soul of the world is respiration; in respiration we breathe in the fire and therefore the rationality diffused in the air.

Empedocles conceives all natural objects to be mixtures of four original elements — fire, air, water, and earth — which he was perhaps the first thus to distinguish; and to be subject to the action of two forces, one attractive and the other repulsive, to which he gives the allegorical designations of love and hate. In the course of his physical theory he seems to have made no mention of the soul, and nowhere to have dropped a hint that he regards it as a being distinct from the body. But he mentions various psychical activities, and his explanation of them is a consequence of his philosophical theory. The faculty of thought, for example, he explains as consisting in a certain mixture of the substances that compose the blood, and he explains the other faculties in a similar manner.

We find an analogous view in the *Pythagorean school*, certain members of which held that the body consists of two pairs of opposites, the warm and the cold, the dry and the moist, and that the soul is the harmony or appropriate mixture of these. Besides this the Pythagoreans put forth two further views of the nature of the soul. The first is a deduction from their philosophy, whose fundamental thesis is that the essence of the world consists in numbers, or the mathematical properties of things; in conformity with this principle they define the soul as a number endowed with the power of self-movement. The other view is of a religious nature, and closely allied to the tradition propagated in the Orphic mysteries; it regards the soul as an immortal being, imprisoned in the body as a punishment for its sins, and calls the body the tomb of the soul. This view was subsequently taken up by Plato, and gives a characteristic coloring to his whole psychological theory.

III.

To understand the philosophical basis of the psychology of *Democritus*, we must go back to the Eleatics, whose fundamental principle is the exact opposite of that of Heraclitus. Heraclitus is so impressed with the fact of change that he makes it the principle of things; for his fire is simply the personification of restless change. Parmenides, on the other hand, thinks he sees clearly the impossibility of such a thing as change; that one thing should change into another different from itself seems to him to involve a contradiction. He therefore denies the reality of the sensible world, where such changes seem to occur, and affirms that the only reality is all-inclusive "Being," and that "Being" remains forever immutably what it is. The atoms of Democritus are simply the "Being" of Parmenides cut up very fine. Like it, they are ingenerable, indestructible, and immutable. They possess only mathematical qualities, and therefore differ from one another only in shape, order, and position. They are infinite in number, and together constitute the universe, and there is nothing beside them.

Though the soul is distinct from the body, it is impossible on atomistic principles that it should be other than corporeal. But the matter of which it consists must be of a sort to explain its essential properties, which are, first, motion, and secondly, thought. Now motion can only proceed from that which is itself in motion, and the soul must therefore consist of the most mobile kind of atoms; these are the very fine, smooth, round ones that constitute fire. Democritus thus agrees with Heraclitus that the soul is of the nature of fire. This harmonizes well with the second essential property of the soul, thought; for thought is itself a subtle kind of motion. The fiery atoms that constitute soul are diffused through the entire body, in such a way that between every two body-atoms there is a soul-atom. The body is alive in all its parts, because in all parts there are atoms which by virtue of their nature are in continual motion, and which therefore set in motion the atoms that surround them. But the motion of the soul-atoms is not the same in every part of the body; that is to say, the different psychical activities have their seat in different parts, thought in the brain, anger in the heart, desire in the liver. Since the fiery atoms that constitute soul are everywhere diffused in space, the whole world must be alive; yet not in the sense of a unitary being. "There must be much soul diffused in the air, how otherwise could we breathe in life and soul out of it?" The preservation of life depends upon the uninterrupted accession of new soul-atoms from without in breathing. For since the soul is not completely enclosed by the body, some of its atoms are continually escaping; the surrounding air presses them out of the body because of their smallness and fineness. Breathing not only introduces new soul-atoms into the body, to replace those that are lost, but mechanically obstructs the exit of those that remain. When breathing ceases, the last obstacle to the escape of the soul-atoms is removed, and their departure is what we call death.

The fact that this theory is materialistic does not prevent it from being an almost perfect reproduction of primitive animism; for animism itself is vaguely materialistic. The theory of atoms is the only novel feature. We may point to the following elements as distinctly animistic: the view that the soul is not immaterial, but only a more ethereal kind of matter than the body; the view that in breathing the soul receives nourishment from without; the view that the soul lives in the body while breathing continues, and disappears with the breath; the view that the soul is identical with the bodily warmth.

In the theories we have thus far considered, the common tendency has been to regard the soul as a refined form of matter. We cannot say that *Anaxagoras* gets wholly beyond this view, for he says that mind is unlimited, that it is the finest and purest of all things, and that in different objects there are greater or smaller portions of it. Nevertheless, if he still conceives mind as a form of matter, he manifests a clear insight into the radical difference that separates this form of matter from every other form. It is interesting to observe the characters by which he seeks to differentiate mind-stuff from ordinary matter. Ordinary matter is a mixture of all things, containing particles of flesh, blood, vegetable tissue, gold, silver, etc., indiscriminately mingled together. In fact, every object contains particles of all existing substances, and receives its name as this or that only from those that predominate. "All things are together, everything is in everything else." In contrast to the indiscriminate mixedness and mutual dependence of ordinary matter, there must have existed from the beginning a special kind whose privilege it was to be absolutely independent and unmixed; for only that which is unmixed can have power over and know that which is mixed. This is what Anaxagoras calls *nous* or mind. Since mind has no constituents, it can have no qualities or differences within itself, but must be homogeneous throughout; while ordinary material things differ from one another according to their composition. Only by having no qualities can it *move* other things; for if it had qualities, it would be one among things, and they would then act upon it, and its dominion over them would not be absolute. Only by having no qualities can it *know* other things; for if it had qualities itself, these would dim its vision and prevent it from perceiving clearly the qualities of things. Mind is thus altogether active in its nature, and alone able to act without being acted upon. When it acts upon matter, it causes motion; in fact, mind is the original source of all motion. We might suppose motion to be uncaused and to be equally original with matter, were it not for the order and beauty which pervade the world, and which come about by means of motion; but these show that motion must have proceeded from some spiritual source, that is, from mind. Mind is thus the ruling element in the world, it "knows all things and has the greatest power."

It will be evident that the psychology of Anaxagoras marks the widest departure from animism we have yet encountered.

IV.

It does not enter into the plan of this sketch to present in detail the views of the pre-Socratic philosophers regarding the physiology and psychology of the senses; but we may insert at this point an account of the most ingenious and suggestive of these theories, that of Democritus, which will serve as a specimen of their general character.

In order that sensations may take place, it is necessary, according to Democritus, that portions of the external object should come in contact with the sense-organs, and that the impression there produced should be propagated into the interior of the body and communicated to the atoms of soul. The impression on the sense-organ must have a certain intensity, that is to say, the parts that touch the sense-organ must have a certain density and solidity, otherwise no sensation arises; tones, for example, find access to the soul through every part of the body, but are heard only through the ears, because it is only through these that they find access in sufficient quantity. Democritus regards the sense-organs as merely passage-ways for matter; thus the essential feature of the eye is its moist and spongy character, and the ear is only a tube which admits the vibrating air into the body.

Visible objects give off effluences, which are images or copies of themselves in miniature, as it were their peeled-off surfaces. These images are complexes of atoms, like the objects from which they come. The essential thing in vision is, first, that these images should be reflected in the eye, and secondly, that the impressions thus produced should be propagated as far as the atoms of soul. Strictly speaking, it is not the images that leave visible objects that are reflected in the eye; for the space between objects and our eyes is filled with air, and the air is densified by the warmth of the sun, and this obstructs the passage of the images themselves; so that what actually reaches the eye and is reflected there is the likeness of the images impressed upon the densified air. This is why we see indistinctly at a distance; if the space between objects and our eyes were empty, we should be able to see an ant in the sky. A second hindrance to vision is the fact that effluences are continually given off by our eyes. It follows that we do not perceive things just as they are, but that the qualities of visible objects are partly subjective. Color is a purely subjective phenomenon, the objective cause of which lies in the mathematical qualities of things, which are the only qualities they possess in themselves. Here we have the earliest statement of the distinction between the primary and secondary qualities of matter. There are four fundamental colors — white, black, red, and green — all others are mixtures of these.

Passing to the sense of hearing, a sound or a tone is defined as a stream of atoms proceeding from a sonorous body. This stream sets in motion the air that lies before it, and thus produces a current consisting partly of atoms from the sonorous body, partly of atoms of air. In this current the atoms that have the same size, smoothness, roundness, and fineness drift together. The stream enters the body through the external passage of the ear and penetrates as far as the atoms of soul; when it acts upon these, auditory sensations arise. A tone is purer in proportion to the homogeneousness of the atoms that compose the stream, higher in pitch in proportion to their smallness and fineness.

When certain of the effluences of things are inhaled into the nostrils, we have sensations of smell. A sweet taste is due to large, round atoms, which penetrate through the entire organism and affect it everywhere mildly and pleasantly. A sour taste is caused by rough, angular atoms; the taste of fat by small, thin, round ones. The atoms of a white object are "like the inner surface of a shell," that is, they are well-rounded, and they cast no shadows; those of a black object, on the contrary, are rough and uneven, and cast shadows. Objects that are smooth to the touch have their atoms regularly arranged; rough objects have their atoms irregularly arranged. The subjective qualities that objects present are due not only to the character of their atoms, but also to the quickness or slowness with which the effluences move, and the momentary density of the air through which they move.

Before passing to the psychology of Plato and Aristotle, we may devote a few words to the epistemological views of the pre-Socratic philosophers, which throw an instructive light upon their psychological method.

Heraclitus, Empedocles, and Democritus agree in subscribing to the theorem that like is known by like. Heraclitus states generally that we know the external fire by means of the fire within us. Empedocles goes farther, and holds that our bodies contain water, earth and air as well as fire, and that we know air by means of the air in us, water by means of the water, and earth by means of the earth: in short, his assumption is that, if the external world is to be known, its constituents must also be those of the knowing subject. This theory may have found confirmation to his mind in the observation that the individual senses resemble the objects they are fitted to perceive: thus the eye not only perceives shining objects but shines itself, the air in the hollow of the ear resembles the external air, the skin is solid and resisting like the objects it touches, etc. Empedocles further states that the blood is adapted to be the substratum of thought because it contains all the elements mixed together, and that this is especially true of the blood in the neighborhood of the heart. Democritus adopts the same view when he says that we perceive everything with that part of our nature which is allied to it, and that the closer the resemblance between the two the more exact the sensation. He draws from this view a couple of sagacious inferences: that there are probably many things which we do not perceive because they are not suited to our senses; and that other beings may have senses which we do not possess.

Anaxagoras, on the other hand, is obliged, in consistency with his fundamental principle, to break with the theory that like is known by like; for mind is altogether unlike the material things it knows. Now one thing acts upon another by changing it into its own likeness; like therefore makes no impression upon like, for it produces no change in it; only unlike can act upon unlike in such a way as to alter it. Knowledge therefore depends upon the percipient subject being unlike the perceived object. Anaxagoras thus appears to recognize that sensation is a qualitative change of the subject, which cannot be produced by that which resembles the latter. He has not far to seek for observations to bear out this view; for instance, temperature sensations depend upon the skin being either warmer or colder than the object. He applies the same analogy to vision: vision, he says, consists in a reflection of the object in the eye-ball, but the background upon which it is reflected must be of a different color from the object, and this is why we cannot see in the dark, for all objects are then of the same color as the interior of the eye. Unlike, then, is always perceived by unlike. Since everything is in everything else, our bodies must contain particles of all possible substances; and this enables us to perceive every quality of external objects by means of its opposite in us — the rough by means of the smooth, the bitter by means of the sweet, etc.

Parmenides, Heraclitus, Empedocles, and Democritus all distinguish expressly between perception and thought, and give the preference to thought as the only trustworthy source of knowledge. Parmenides does so because the senses make it appear as if there were such a thing as change; Heraclitus, because they make it appear as if there were such a thing as permanence. Even Democritus, in spite of the consistent materialism of his theory, is obliged to recognize the superiority of thought; for the atoms of which all things are ultimately composed are imperceptible to sense, and must therefore be known by means of some higher faculty. Yet he says that the difference between perception and thought is only one of degree, the knowledge of the intellect being merely more acute than that of the senses; for both consist solely in material changes, and are produced in the same manner, by means of mechanical impressions from without. Indeed, it is difficult to see from the account he gives how perception and thought are differentiated from each other. Yet Democritus thinks very differently of their worth; sensible knowledge, he says, is "dark," the only genuine knowledge is that of the intellect.

V.

The distinctive position of *the Sophists* is that of scepticism in regard to the possibility of objective knowledge. Democritus had observed that colors, sounds, etc., are affections of the subject, not qualities belonging to material things in themselves. The Sophists generalize this observation, and maintain that all qualities and attributes without exception, in short the total content of knowledge, is merely a subjective state. This insight leaves it an open question whether the content of knowledge reproduces the actual relations of things. It is possible to doubt whether it does: and this doubt is Sophisticism.

One of the principal Sophists, *Protagoras*, bases his theory upon Heraclitus' doctrine of eternal flux. The universe, he says, consists of nothing but a vast multitude of colliding motions. Every sensation is the result of two such motions; a color-sensation, for instance, arises when a motion approaching the eye from without collides with the motion that constitutes the glance of the eye. And every other state of mind is produced in the same way — pleasure and pain, desire and fear, knowledge and thought. It follows that things exist only as they appear, and that as they appear to every man, so they are. That things exist apart from appearances, is an assertion that cannot be substantiated.

Plato relates in the Phaedo that *Socrates* had accepted as a youth the traditional notion of the soul, but had subsequently lost confidence in it. He would always ask "whether it is the blood by virtue of which we are rational, or air, or fire?" The explanations of the philosophers were so unsatisfactory, that he resolved to abandon psychological and cosmological investigation, believing it a waste of time, and ever after expressing contempt for a knowledge that had no bearing upon action. Socrates agrees with the Sophists in holding that knowledge is a state of the subject, but declines to draw the conclusion that universal and necessary knowledge is therefore impossible. He holds that side by side with our perceptions we have mental states of a different kind which enable us to distinguish between the true and the false — namely, concepts or class-ideas. The business of philosophy is the investigation of these and the determination of their proper content; and we read in Xenophon and Plato how Socrates would discuss the meaning of beautiful and ugly, just and unjust, pious and impious, the essence of prudence and folly, the nature of the family and of the state, etc. He believed that all men, when they think consistently, have identical concepts about such matters, and that therefore, however their perceptions may differ, they have objective knowledge, at least in moral matters, by virtue of their concepts.

The philosophy of *Plato* is the first great systematic expression of that tendency of thought which places mind before matter as the first principle of things. Such a philosophy involves the consequence that the ultimate ground of all existing things must be sought in the domain of ethics; and so it comes about that the psychological views of Plato are largely influenced by ethical considerations.

The work of previous philosphers has left the postulate that the ultimate ground of existence must be unitary, in contrast to the multiplicity of phenomenal things, and constant, in contrast to their ceaseless flux. The concepts of Socrates seem to Plato to suggest a better hypothesis in regard to the ground of existence than any yet proposed. For the unity of the concept contrasts with the multiplicity of the individual objects to which it applies, and its fixity contrasts with their endless variability. Furthermore, concepts form an articulate system, the lower being included in the higher, and a highest concept including all the others. Now perception merely reveals to us the outward shows of things, while thought acquaints us with their inner reality. If then, every percept corresponds to some external reality, how much more must every concept have a reality corresponding to it? The realities that correspond to concepts are the Platonic Ideas, and the highest Idea, which includes all the others, is the Idea of the Good.

Our two faculties of perception and thought thus reveal to us two disparate worlds: perception reveals a world of individual objects, where all is multiplicity and change; whereas thought opens up to us a realm of supersensible essences, which together constitute a unitary spiritual being, the Idea of the Good. Since the relations of the Ideas correspond exactly to the relations of the concepts by which we know them, we can find out all about reality by turning our attention inward and investigating the mutual relations of our concepts. Here we have the great original of the *à priori* type of philosophy, which disdains experience and undertakes to discover truth by the effort of unaided thought.

It might be expected, since all souls form a class, that Plato would recognize an Idea of the soul. But since the contrast between the Ideas and their individual copies is that between the eternal and the transitory, this would be equivalent to denying the immortality of the soul, which his ethical interest forbids. He therefore assigns to the soul a middle position between Ideas and individual things. He says, moreover, that though there is no Idea of the soul, there is an Idea with which it is indissolubly connected, namely the Idea of life; which is as much as to say that the soul is the principle of life. Plato believes that the world as a whole is animated by a soul. For the world is a copy of the Idea of the Good, and must therefore be as perfect as possible; now what has reason is more perfect than what has not, but only soul has reason: hence the visible world must have a soul. Hence, in the Timaeus, Plato calls the visible world "a blessed god."

Plato's statements regarding the nature of the soul are made in the first instance with reference to the world-soul. He says that the soul existed first, and the body was formed afterwards, thus recognizing the priority of the soul to the body. The soul is not a mere harmony of the body, as the Pythagoreans maintained; if it were, it would perish with the body. It *has* harmony, but that does not exhaust its being. It is a substance, diffused in harmonious proportions throughout the visible world. The essential qualities of this substance are simplicity, invisibility, and incorporeality. The activity of the soul is twofold, consisting partly in motion and partly in knowledge. The soul is the original source of motion, for it alone is self-moving; all other things receive their motion from without, but the soul moves itself, and in moving itself moves the body. The soul knows all things, for it has the most perfect kind of motion, the circular, by which it "returns into itself and informs itself of everything it has met in its course." All inferior beings have their soul by participation in the world-soul. The highest individual souls are those of the heavenly bodies, next come the souls of men. The end for which the human soul exists is the attainment of rational knowledge, for the soul is by nature "fond of learning." "As the eye is fitted to perceive the sunlight, so is the soul to contemplate the Idea of the Good." Now the contemplation of the Idea of the Good is possible only by rising above sense-experience, and sense-experience has its source in the body. The body is thus little more than a hindrance to the soul; it is a misfortune to the soul to be imprisoned in it; the body is the grave of the soul. The soul did not always dwell in the body, but descended into it from a former celestial state; in its proper nature it has no need of the body; it lives best and happiest when it pays as little attention to the body as possible. For the proper activity of the soul is thought, or the contemplation of ideas, and to this the body can be no help, but only a hindrance.

Since the soul existed before its union with the body in a better state, in which it was entirely occupied in pure thought, it is evident that the sensible part of the soul does not belong to its real nature. Plato therefore divides the soul into two parts, one immortal, the other mortal; and the latter he subdivides into two, a nobler and an ignobler part. We thus have three parts of the soul:

1. A rational or immortal part, whose activity is thought.
2. A nobler mortal part, to which belong courage, anger, love of power, and in general the better and stronger states states of feeling.
3. An ignobler mortal part, to which belong pleasure and pain, and all the sensual appetites and passions.

In proof that these three are not merely distinct forms of activity, but separate parts, Plato instances the fact that desire is sometimes at strife with reason, and sometimes fights on its side: activities so independent of each other must spring from separate causes. How this trinity of parts is to be reconciled with the unity of the soul, Plato does not explain; they are in reality three connected beings, not one being. Each of the three parts has its special bodily seat. That of thought is the head, and the senses are the instruments it employs; that of courage is the breast, and particularly the heart; that of desire is the belly. The liver is the seat of imagination, by means of which reason rules desire; "upon its polished surface reason causes now terrifying, now diverting images to be reflected, she alters its natural sweetness and color by the introduction of bile, and so either frightens or soothes the appetitive part into obedience." But the soul is also mingled with the spinal marrow. That part of the marrow which is rounded into a ball and enclosed in the skull contains the divine part of the soul; the remainder of the marrow contains the mortal part. Both of these parts are ensheathed in a case of bone, the lower end of which is connected by a tube with the passage for drink, by which liquids pass through the lungs and into the bladder; through this tube the seed makes its escape, for the seed comes from the marrow and therefore contains soul.

The sense-physiology of Plato marks no advance beyond that of Democritus. He observes that as a rule only movable parts have sensation; those that cannot be voluntarily moved, as for example bones and hair, are insensitive. Sensation arises whenever an external impression communicates a motion to the body, and this motion is propagated as far as the soul. As to what conducts these motions, Plato thinks it is the blood in the blood-vessels, owing to its mobility; for both Plato and Aristotle are unacquainted with the nerves. If the motion of the blood takes place very gradually, no sensation is produced; if it takes place quickly, but easily and without obstruction, we have clear perception, without pleasure or pain ; but if it causes a sensible raising or lowering of the general state, in the one case pleasure arises, in the other pain. Sensations of smell arise when vapors penetrate into the blood-vessels between the head and the navel, and cause there either a rough or a gentle motion. Sensations of taste depend upon the contraction or dilatation of the blood-vessels of the tongue. Auditory sensations arise when external sounds set in motion the air in the interior of the ear. Visual sensations are produced when the fire emitted by the luminous body collides with the fire that dwells in the interior of the eye. Finally, Plato distinguishes from sense-perception the faculty of thought, by means of which we compare our sensations, recognize their relations to one another, and infer from them the existence of actual objects ; but the highest exercise of thought is the contemplation of the divine Ideas.

VI.

Aristotle aims in his Metaphysics to explain the universe by indicating the principles that enter into its construction. He finds that these are four in number: first, the material cause, or stuff of which the universe is composed ; secondly, the formal cause, or idea of which it is a realization ; thirdly, the efficient cause, or motive force which brought it into being ; and fourthly, the final cause, or end which its existence subserves. These four principles — matter, form, motive force, and end — must coöperate to produce the universe as a whole or any portion of it.

If we try to classify these principles with reference to the source from which they come, we have on the one side matter, which must in every case be furnished from without ; and on the other side the ideal form, the efficient act, and the purpose or end, all of which have their origin in the mind. It thus happens that these last three tend to coalesce into a single principle called Form, which stands opposed to matter as that which originates in the mind to that which originates from without. Matter, though the raw material out of which all existing things are formed, is in itself formless and chaotic ; it is likewise wholly passive and unable to move itself. Form, on the other hand, is the principle of activity and the original source of motion. It is that which, by supervening upon unformed matter, transforms it into a concrete object, and may therefore be said to be the ideal of the concrete object it subsequently becomes. In fact, it is Aristotle's substitute for the Platonic Idea, and is even occasionally called by the same name. But there is an important difference between the Platonic

Idea and the Aristotelian Form. Plato represented the Ideas as existing apart from concrete objects in a transcendental realm of their own; Aristotle maintains that they must be immanent in the objects themselves. Only individual things, according to Aristotle, are real in the proper sense of the term; Forms or Ideas, apart from the individual things in which they are realized, are mere figments of the imagination. As in Plato, the ideal or Idea of an individual object is identified with the generic in it, that is, with that assemblage of traits which makes it member of a class. But what is most foreign to our habits of thought is the assumption that the generic in an individual object is not a mere part of its description, but the active force that causes all the changes it undergoes. Change, on this view, consists not so much in Matter taking on new Forms, as in the Forms successsively taking possession of Matter. Matter already contains within itself potentially all the various things it is capable of becoming; when the Forms supervene upon it, this potentiality becomes an actuality. Change may therefore be conceived as the transformation of a potentiality into an actuality.

We are now in a position to understand Aristotle's definition of the soul and his view of the relation between it and the body. Plato may be said to have conceived the union of soul and body as the spatial juxtaposition, as it were, of two independent substances. Aristotle, on the other hand, regards man as an organic whole. He says that the nature of each part must contain the reason for its union with the other. Of this organic whole the soul is the more significant side, yet we cannot say that the soul is the true man and the body a mere appendage. The soul does not think, feel, learn, etc., but the man does so by means of his soul. Old age, illness, drunkenness, etc., are not states of the soul alone, nor of the body alone, but of the unitary being that soul and body constitute. The soul is primarily the cause of life. Now life consists essentially in the power of self-movement, in the capacity of a being to produce changes in itself spontaneously, whether these changes are of a gross character visible to the eye, as in locomotion, or are limited to the minute internal movements that constitute nutrition and growth. We thus have a body in which changes are produced, and an inner principle which produces them, and the relation of the two is exactly that between Matter and Form. Since the body cannot move or change itself, it must be of the nature of Matter; and the soul, as that which moves and produces changes in the body, must be of the nature of Form.

Since all matter is the potentiality of that which it can become, the body must be the potentiality of a living being;

and since the soul is that which transforms this potentiality into an actuality, the soul may be described as the *entelechy* or actuality of a living being. But the word *entelechy* is ambiguous, for it may mean either the actualizing agency, or the activity of actualization. The soul is an *entelechy* in the former sense, for it exists even during sleep, when its functions are suspended; the soul is thus the ever-present possibility of the functions of life. To indicate that the soul is an *entelechy* in the sense of an actualizing agency, Aristotle calls it the *first entelechy*, and his definition therefore reads: the soul is the first *entelechy* of a natural organic body that has in it the potentiality of life. Since the soul is the form of the body, it must be immaterial; but, though immaterial, it cannot exist apart from matter. Indeed, Aristotle mentions a special kind of matter with which the soul is directly connected, and with which it passes in generation from one being to another. This he sometimes designates as warmth, sometimes as the breath; he describes it as of nobler nature than the four elements, and as resembling ether. The solution, then, of the problem, how soul and body can constitute a single being, is that the two belong to totally different orders of existence. Like form and matter everywhere, they are distinguishable in thought, but they cannot exist separately, any more than the eye and vision can exist separately.

Plato not only distinguished three parts of the soul, but assigned to them separate seats in the body. Aristotle raises the question whether such spatial separation can consist with the unity of the soul, and decides that it cannot. He therefore contents himself with enumerating the classes into which psychical manifestations fall, and distinguishes four:

1. Nutrition and growth — the nutritive part.
2. Sensation and perception — the sensitive part.
3. Desire and locomotion — the locomotive part.
4. Thought — the rational part.

Of these four, plants possess only the nutritive soul; animals have this and the sensitive soul as well. The lowest kind of sensation is touch, and this all animals possess; with touch go always pleasure and pain. Most animals have locomotion as well as sensation, and with this desire. Man, finally, possesses in addition to the nutritive, the sensitive, and the locomotive soul the highest form of psychical activity, rational thought. It thus appears that the lower parts can occur without the higher, but not the higher without the lower. Notwithstanding these various forms of psychical activity, it is the same identical soul which manifests itself in them all. If the soul consisted of several juxtaposed pieces, it would be held together by the body; whereas in reality the body is held together by the soul.

The central organ of psychical activity is the heart, not the brain, whose function is merely that of cooling the blood. The heart is the organ both of sensation and of locomotion. Tactual sensations are propagated to the heart through the flesh, all others through certain "channels," by which Aristotle undoubtedly means the blood-vessels, for he knows nothing of the nerves. The heart also causes the movements of the limbs.

Sensation is the most distinctive characteristic of the animal as compared with the plant. It consists in "an alteration brought about by the perceived object in the percipient subject through the medium of the body." This alteration is of such a sort that the percipient subject, functioning as matter, takes upon itself the form of the perceived object. Sense-perception may therefore be defined as the reception of the form of an object without its matter. The action of objects upon the senses always takes place through some medium; in the case of vision this medium is light, in that of hearing air, in that of smell moisture. In touch and taste there seems to be no medium, but there is one, namely the flesh, and the true organ of these senses is therefore not the skin, but the heart. The fact that by touch we perceive so many pairs of opposites—hard soft, rough smooth, dry wet, warm cold—suggests to Aristotle the doubt whether it is really a single sense; which he silences with the remark that the other senses also perceive more than one pair—hearing, for example, perceives not only differences of pitch (high low), but also differences of intensity (loud soft), roughness and smoothness of voice, etc. The senses of touch and taste are so indispensable to existence that all animals possess them. They are the lowest senses, for they minister to the lowest functions, those of nutrition and reproduction. Sight and hearing, on the other hand, stand highest, for they are the means by which the intellect is developed; and of the two, hearing is the superior sense, because upon it the communication of ideas by means of language depends. Aristotle endeavors to prove, as against Democritus, that it is impossible there should be other senses than the five.

Each of the senses yields us a kind of sensation peculiar to itself alone; for instance, vision alone yields color, smell alone odor, etc. But there are other qualities of objects which are common to the perceptions of all the senses, namely such universal qualities as number, size, and form, motion and rest, and time. Now we cannot suppose, to take an example, that the space we see is a different space from that we touch; but if they are one and the same space, they must be perceived by one and the same faculty; and therefore not by sight nor by touch, but by some deeper-lying faculty which functions in connection with both these senses. This faculty Aristotle calls the *sensus communis*. It is this faculty by which we compare and distinguish the data of the different senses, for no single sense can compare what it perceives with what is perceived by some other sense. It is this faculty which considers our sensations as representing something objective, for the individual senses cannot judge of this, but can only feel what they feel. It is this faculty, finally, upon which self-consciousness depends, for "sight perceives only what is colored, and if sight perceived seeing, seeing would itself have to be colored."

The *sensus communis* is also the faculty of imagination and of memory. Imagination is an after-effect of sensation, a weakened form of sensation. For the commotion produced by the original impression persists in the sense-organ, and when it is again propagated to the heart, it causes a revival of the original sensation in the absence of the object that caused it. When this image is not only revived, but regarded as the copy of the previous sensation, we have memory; and the voluntary reproduction of a memory is recollection. Recollection is rendered possible by the fact that the organic motions which accompany the images of memory have a mutual connection, of such a sort that one calls up another; and the reason for this connection may lie in their similarity, their contrast, or their previous conjunction in time. Imagination, finally, furnishes the visions we see in our dreams, as well as the images that accompany thought.

Thought, or reason, is the highest of the mental faculties, and is that which distinguishes man from the lower animals. Though distinct from sense-perception, it deals with the same objects, that is, with the images which sense-perception furnishes. But it is concerned with a different aspect of them from that which occupies sense-perception, namely the generic in them. It is also concerned with relations such as likeness and unlikeness, cause and effect, form and matter, etc. Aristotle distinguishes two kinds of reason, the active and the passive, corresponding to the distinction that everywhere obtains between form and matter, and teaches that the cooperation of these two is necessary to actual thought. Thought as matter he calls the passive reason; thought as form his later followers call the active reason. To understand this somewhat difficult distinction, we must recur to Aristotle's explanation of sense-perception. As in sense-perception the human faculty, functioning as matter, takes on the form of the external object, so in rational thought the human faculty, or passive reason, as matter, unites with the conceptual relations that are immanent in our percepts, as form, and it is these conceptual relations which Aristotle designates as the active reason. When the two unite, actual thought is the result. The strangeness of this distinction lies in the fact that Aristotle attributes the activity that manifests itself in thought to the content thought about. From this point of view it would seem less correct to say that we think thoughts than that thoughts think us. Aristotle says that the passive reason comes into existence with the body and perishes with it, and during life participates in its states. But the active reason has nothing to do with the life of the body; has no bodily organ; does not come into existence by procreation, but enters the body from without; and is therefore unaffected by the destruction of the body. The immortality of this part can have, however, little worth for the individual, for it possesses neither memory nor self-consciousness.

Though Aristotle's statements regarding the active reason may seem to mark a relapse into dualism, yet his psychology as a whole is distinctly monistic. He conceives the development of the soul as running parallel to that of the body, and his method is a biological-developmental one. He is a keen observer of mental phenomena as well as a profound metaphysician; he brings to bear upon psychology as much of anatomy and physiology as was known in his time; and he everywhere brings human into fruitful relations with animal psychology. Finally, he delivers psychology from the premature influence of ethics, recognizing that ethics depends upon psychology, not psychology upon ethics. It is these merits which make Aristotle the greatest psychologist of antiquity.

STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.

BY JOSEPH JASTROW, PH. D.

I.—THE EFFECT OF FOREKNOWLEDGE UPON REPETITION—TIMES.

(With the assistance of FREDERICK WHITTON.)

The experimental contributions to the study of the effect of foreknowledge upon the times of simple mental processes may be thus briefly summanized. In simple reactions the nature of the stimulus is of course foreknown, but the precise moment of its appearance and its intensity may be left indefinite. It has been found that the omission of a preparatory signal, or an irregular interval between signal and stimulus, as also are irregular variation between more or less intense stimuli, all lengthen the simple reaction-time. In that form of a distinction-time, in which one particular stimulus is to be reacted to but all others are passed without reaction, it is found that the larger the number of possible stimuli (and therefore the less definite the foreknowledge) the longer the reaction-time. In adaptive reactions, with the number of modes of reaction constant the time will be longer as each mode of reaction is connected successively with one, with two, with three or with more and indefinitely many stimuli; the stimuli may or may not be grouped in classes. In association-times Münsterberg has shown that the preceding of a question asking for a personal preference or judgment between a pair of objects, by the mention of a dozen or so of the class of objects to which the pair belongs, decidedly shortens the time of answer to the question, in one series from 947 σ to 676 σ.[1] This last form of experiment is extremely interesting; it seems to show that although we cannot begin to say, for example, whether we prefer peaches to pears, until we have heard the full question,—"apples, plums, cherries, peaches, grapes, oranges, pears, figs, lemons, dates, apricots, pine apples,—which do you prefer, peaches or pears?,"—yet the time needed for this decision is much shorter than when the introductory series of words is omitted.

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[1] For a more detailed account of these points see Jastrow, Time-Relations of Mental Phenomena; pp. 15-17, 39-40, 50-51, etc.

The object of the present study was to test this point in a much more simple type of reaction, and with a variable number of possible stimuli. We selected for this purpose the repeating aloud of spoken words, the operator called a word and as quickly as possible the subject repeated it, all the words used being monosyllables. We found as the average of about 250 experiments with each of us that the time needed for doing this when the word might *be any word whatever*, for J. J. 269$^{\sigma}$ for F. W. 267$^{\sigma}$. We formed lists of words as follows: (a) 100 very common verbs, signifying simple actions; (b) 50 common names of animals; (c) 20 proper names, such as John, Frank, Bess, Kate; (d) 20 letters (omitting b, d, m, n, v, w, as confusing in sound or polysyllabic); (e) 10 common French words; (f) the ten numbers, 'one,' 'two,' etc. to 'ten.' Only one list of each class of words was used, so that we became increasingly familiar with the lists. Before each set of 20 experiments the entire list of words from amongst which the words for repetition were to be selected, was read aloud. The following table shows for each of us the average time needed to repeat words under these circumstances. Each result expresses the average of from 240 to 300 experiments.

The conclusion thus corroborated is that *as the range of possible words decreases in extent, as the subject's expectation is more and more definite, the time needed to repeat the word becomes shorter*. It indicates the power and the utility of a general direction of the mind in the line of a more specific operation; the entrance into the general field of attention as preparatory to entering its fovea; the apperception that precedes preception, or in Galton's words, the entrance into the ante-chamber of consciousness to prepare the way for admission to the audience-chamber. We have here an adaptive reaction, each different word forming a distinct stimulus and the vocal manipulation necessary to repeat it a distinct form of reaction. It would seem that we could not get ready to repeat a word until we knew what the word is, and yet a knowledge of the possibilities of the case really aids our expectation and shortens even so simple a process as repetition. We perform the coarse adjustment before the stimulus appears, leaving only the time of the fine adjustment to be measured. There exists all degrees of definiteness and indefiniteness of expectation, of fore-knowledge, and an increase of definiteness to a certain limit brings about a shortening of the mental processes of recognition and adaptation.

While the results are too few and too variable to admit o any detailed treatment a few more special points may be pointed out as suggested though not as established. The extreme regularity of the results, the gradual decrease from repetitions of one of an indefinite number of words, to one of 100, of 50, of 20 and of 10, is doubtless accidental; the times for repeating one of 50, one of 100 or one of an indefinite number of words, for F.W. and of the latter two for J. J. are practically the same, and indicate a limit to the range of expectation. To expect one of 100 words seems scarcely a more definite attitude than to expect any word whatever. With F. W. this seems true of 50 words as well. It seems clear that it takes less time to repeat one of 20 words than one of 50 words, and least to repeat one of the ten numbers. We know the numbers so well as a class and as a series that expectation is here most definite. A French word on the other hand is relatively unfamiliar, and it takes longer to understand and repeat it. To obtain the time needed for the mental portions of the process alone, we substract the simple reaction-times from the repetition-times. How the former was obtained will be explained in a note.

Dr. Münsterberg has attempted to carry the distinction between the "motor" and the "sensory" form of reaction into complex types of reaction; indicating by the former a more special attention to the modes of reaction, by the latter a more special attention to the stimulus. Dr. Martius attempted to repeat the experiments in every detail but failed to obtain the distinction. We found it difficult to maintain this difference of attitude in repeating words, and the results (see the table above) show practically no difference in our cases between the two forms of reaction; the average of all the "motor" experiments was for J. J. 245 σ, for F. W. 249 σ, of the 'sensory' for J. J. 249 σ, for F. W. 251 σ. Even in the simple reaction the difference is slight; but in the ordinary reaction with a finger-key one of us shows the difference. J. J.'s simple reaction to a sound by closing a key with the finger is 136 σ for 'motor,' 162 σ for 'sensory'; F. W.'s 133 σ and 137 σ. While these results have probably only an individual significance, yet in our present incomplete knowledge of the true nature of the distinction between 'motor' and 'sensory' reactions, they may be worthy of record. *Note upon apparatus and method.* Our apparatus and method were gradually perfected during the course of the experiments (covering a period of eight months) and only such of our results were included in the averages given above as were obtained by the same method and seemed fairly comparable with one another. We began by attempting to speak the word and press the key with the finger at the same moment, the subject also repeating the word and pressing the key as nearly as possible at the same moment. (We used keys of the form to be described in the next note, but later to avoid the noise the caller used a mercury key). This is also Münsterberg's method. We soon found a very strong tendency to close the key too soon, on the part of the reacter, and too late on the part of the caller; the former presses the key when the voluntary impulse is ready, when he feels that you know what the word is and what it is necessary to do to repeat it, rather than when the vocal mechanism is ready and may act. By this method our times were much too short, centering about 200 σ. The simple reaction time to a sound by closing a key with the finger was for J. J. 148 σ, for F. W. 135 σ. But it is hardly proper to subtract this from the repetion time to obtain the time of the mental process alone. To include the complete mechanical process the stimulus must be a vocal utterance with an accompanying closure of the key with the finger and the same for the reaction. After much trial we conclude to use a small bit of wood held between the teeth and attached to a spring lever, so that the slightest separation of the teeth, (always accompanying utterance) would cause the bit to fly out of the mouth and in so doing to make or break the chronoscope circuit. While the key is not free from objections, it worked very well and we could observe with it no serious tendency to anticipate the true reaction. The simple reaction-times recorded in the table were obtained with this key in the following manner: the observer always uttered the sound "ah" (explosive) and the reacter always used the same sound in reacting, so that the simple reaction includes all the mechanical parts of the process, and whatever error there is in uttering or repeating is contained in all alike. The difference between this and the repetition-time (on the average 68 σ) may thus be regarded as the pure mental repetition process. The further details of method and apparatus offer no peculiarities worthy of record.

A Novel Optical Illusion.

(With the assistance of G. W. MOOREHOUSE.)

If before a rotating disc composed of a large sector of one color and a small sector of another, the two differing considerably in shade (e. g. a dark blue and a light yellow), a rod, held horizontally, be passed up and down, the whole disc seems broken up by horizontal parallel bands of a color similar to that present in greater proportion[1]. This illusion is especially striking when the component colors are markedly different, with the lighter color forming only a very small portion of the disc, when the disc is in very rapid rotation, the rod very slender and its motion moderately rapid. The bands appear quite as well if the movement of the rod is vertical, oblique, rotary, etc.; the effect of bending the rod into a spiral or other fanciful shape and giving it a rotary movement is especially striking, the bands always following parallel to the outline of the wire. If instead of showing but two colors thė disc is composed of three or more the bands appear each composed of several colors; and if a disc composed of small sectors of the seven primary colors be rotated each band presents a rainbow-like appearance. This phenomenon seems especially remarkable when contrasted with the universal tendency of successive optical images to fuse. The mixing of colors upon a disc is itself a typical instance of such fusion. But here there is a sort of separation of colors, and that too at a high rate of rotation. For example, if two rotating discs were presented to us, the one pure white in color, and the other of ideally perfect spectral colors in proper proportion, so as to give a precisely similar white, we could not distinguish between the two; but by simply passing a rod in front of them and observing in the one case but not in the other the parallel rows of colored bands, we could at once pronounce the former to be composite, and the latter simple. In the indefinitely brief moment during which the rod interrupts the vision of the disc, the eye obtains an impression sufficient to analyze to some extent into its elements this rapid mixture of stimuli. The more detailed description and possible explanation of this illusion formed the object of our study as of the present exposition. It will conduce to brevity of description to arrange the several results of experimentation under appropriate headings.

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[1] This illusion was first brought to my notice by Dr. Münsterberg upon my visit to his laboratory at Freiburg. I can find no reference to it in the literature accessible to me.

Extent of the Illusion. The illusion appears with any pair of colors, provided only that the two are moderately different; but the resulting bands are of various degrees of distinctness according to the colors used. The result is clearest when the colors are strongly contrasted; we experimented successfully with red, yellow, blue, green, black, white, etc., in various combinations. Of a series of seven shades of green, numbers "one" and "two" were very dark, number "three" considerably lighter than "two," and the rest all very light with only slight differences between them. The bands could not be observed with a combination of "one" and "two" nor with any combination of "four," "five," "six," or "seven," but in all other combinations the contrast was sufficient to cause the illusion. By a different method, to be described below, we succeeded in more accurately determining the amount of contrast needed to produce the illusion.

Proportions of the Component Colors. In a disc composed of dark red and light yellow, the bands could just be seen when a sector of 12° of red was combined with 348° of yellow, and remained visible with a decrease of yellow and an increase of red until only 3° of yellow and 357° of red were present. With red predominating the bands are also red but of a red *darker* than the general color of the disc; with yellow predominating the bands are yellow but of a yellow *lighter* than the resulting mixture. The darker bands are always more easily seen and clearer than the light ones, and hence a smaller sector of yellow with red than of red with yellow is needed to produce the illusion. We should infer that there would be a ratio of the two colors at which the bands would be neither darker nor lighter than the background; and in fact there is quite a range of ratios for which the bands are so nearly the color of the background that they are difficult to observe. This range differs for different combinations of colors; for our red and yellow the critical point is about 110° of red and the rest yellow. With more red than this the bands become more and more deeply red, and with less red more yellowish; to this extent the statement that the bands are of the color predominating in the disc must be modified.

Effect of the Width and the Rate of Motion of the Interrupting Rod. The general effect of an increase in the width of the interrupting rod was to render the illusion less distinct and the bands wider; moreover the illusion is more limited in range, i.e., it is confined to a narrower range of rotation rates of the disc and the like. While with a fine wire about a millimeter in diameter, the bands are sharply outlined and striking, with a stick 4 mm. in width they require somewhat of a strain to continuously observe them.

Maintaining the rotation rate of the disc as nearly constant as the clockwork that runs it will allow, we may vary the rate of passing the rod to and fro with characteristically different results. Moving the rod across the disc six inches in diameter, so that each movement from up down, and from down up, corresponded with the beat of the metronome beating 208 times per minute, the bands were about $\frac{5}{8}$ inch apart, with the metronome at 160 per minute about $\frac{1}{2}$ inch apart; with 108 per minute $\frac{1}{4}$ in.; with 80 per minute $\frac{1}{8}$ inch; with 60 per minute less than $\frac{1}{16}$ inch. In other words the bands are separated by smaller and smaller spaces as the rate of movement of the rod becomes slower and slower. The distances between the bands were estimated by free-hand drawings and then verified by comparison with the rotating discs.

Analysis of the Factors of the Illusion. Allowing the above to suffice as a general explanation of the extent of the illusion, we may proceed to an analysis of its component factors. The factors are (a) the appearance not of one band but of several; (b) the distance between the bands; (c) the color of the bands; (d) the width of the bands; (e) the color of the interrupting rod; (f) the width of the interrupting rod; (g) the rate of movement of the interrupting rod; (h) the rotation-rate of the disc; (i) the nature and proportion of the component colors. It will thus be seen that the illusion is quite complicated. As an important step towards its explanation, we will consider first,

The Time-Relations of the Illusion. This involves the factors (a), (b), (g), (h). Before proceeding further it will be necessary to describe another method of producing the illusion, without which its explanation would have been impossible. This consists in sliding two half discs of the same color over one another leaving an open sector of any desired size up to 180° and rotating this against a background of a markedly different color, in other words we substitute for the disc composed of a large amount of one color, which for brevity we may call the "majority color," and a small amount of another, the "minority color," one in which the second color is in the background and is viewed through an opening in the first. With such an arrangement we find that we get the series of bands both when the wire is passed in front of the disc and when passed in back between disc and background; and further experimentation shows that the time relations of the two are the same[1]. (There is of course no essential difference between the two methods when the wire is passed in front of the disc.) These facts enable us to formulate our first generalization, viz., that for all purposes here relevant the seeing of a wire now against one background and then immediately against another is the same as its now appearing and then disappearing; a rapid succession of changed appearances is equivalent to a rapid alternation of appearance and disappearance. Why this is so we are unable to say, but the fact itself seems well established, and is both novel and interesting. By this "open disc" method we are enabled to study the illusion independently of color, by having the disc of white against a white background with the rod moving between disc and background. In this case, as in the others, we see several rods or bands, and the suggestion is natural that we are dealing with the phenomena of after images ; in other words we see the rod through the opening in a certain position, then for a brief time lose sight of it, then see it again in a slightly different position and so on, the after image of the one view not having faded out when the second view is obtained. If this is the true explanation of the fact that several rods are seen, then we should—with different rotation rates of disc and rod—see as many rods as multiplied by the time of one rotation of the disc would yield a constant, i.e., the time of an after image of the kind under consideration. The result of about 20 such tests with varying rates of the disc was the following :

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[1] Of course when the wire is passed between the disc and background the distinctness of the wire depends upon its contrast with the background; it appears of its true color modified by its appearance on the background and by the rotating disc through which it is seen, but it does not assume the contrast effects assumed by the rod moved in front of the disc. The time-relations in the two cases are the same but the color-phenomena considerably different.

			J.J.	*G.W.M.*
Average time of rotation of disc when	2	images of the rod were seen	.0812 sec.	.0750 sec.
" " " " "	3	" " " "	.0571 "	.0505 "
" " " " "	4	" " " "	.0450 "	.0357 "
" " " " "	5	" " " "	.0350 "	.0293 "
" " " " "	6	" " " "	.0302 "	.0262(1)"

Multiplying the number of rods by the rotation rate we get for J.J. an average time of after image of .1740 sec. (a little over $\frac{1}{6}$ sec.) with an average deviation of .0057 (=3.2%); for G.W.M. 1492 (a little over $\frac{1}{7}$ sec.) with an average deviation of .0036 (=2.6 %). An indepentent test of the time of after-image of J.J. and G.W.M. by observing when a black dot on a rotating white disc just failed to form a ring resulted in showing in every instance a longer time for the former than for the latter.[2]

It has already been observed that the distance between the bands diminishes as the rotation rate and the rate of movement of the rod increases ; this suggests that the distance between the parallel bands is that moved over by the rod during one rotation of the disc. This we tested with various rates of disc and rod by spreading a pair a compasses until they seemed to span the distance between the bands. The following is a comparison of the actual and the theoretical result under this hypothesis :

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[1] There is a further point to be considered here, viz.: the size of the aperture, when nothing different is said it was 21°. We repeated some of the above experiments, however, with apertures of 10½° and of 42°, obtaining the same results.

[2] For the method of timing the disc see Note A. The rod was moved between parallel bars to the beats of a metronome.

One revolution in x seconds.	Rod moved y mm. in x sec.	Observed distance between bands in mm.
.0551	18	18
.0220	5.13	5
.0227	5.03	5
.00987	1.48	2
.0233	3.04	3.5
.0250	3.05	4
.0376	5.03	6.5

Considering the difficulties of the observation these agreements are extremely close. Having now accounted for the width of the bands, the distance between them, the fact that several are seen, it remains to examine certain general conditions of the illusion and more particularly the color factors of it.

The Color Factors of the Illusion. A brief acquaintance with the illusion sufficed to convince us that its appearance was due to contrast of some form, though the precise nature of this contrast is the most difficult point of all. It has already been observed that the two component colors must be somewhat different to produce the illusion and that the bands are darker when the majority color is darker than the minority color, and is lighter when the former is lighter. By the following device we succeeded in determining the minimum amount of difference between the colors that would produce the illusion: we used an open disc of light green (aperture 21°) in front of a back ground of the same color and used with the green disc a variable sector of black. When moving a rod in front of this combination we always observe a series of light bands due to the presence of the large amount of green with a little black, but as the black gains a certain proportion, we observe in addition a series of dark bands due to the contrast of the resulting darker green (mixture of light green and black) with the light green of the back ground. We have now only to vary the black till these darker bands may just be seen; this critical point with the colors used proved to be about 24° of black added to 315° of green, or “$\frac{1}{13}$ darker” if we may use that expression. It need hardly be added that the aperture exactly corresponds to the minority color and requires no special consideration.

Colors differ in two senses, in the fact that they are formed by different wave lengths, and that they contain more or less black. We have shown that colors differing only in the latter respect produce the illusion; it remains to be seen whether difference of color alone will produce it. We have the following evidence that it will not: (1) We were able to select a dark red and a dark blue, which did not give the illusion, but in which the substitution of slightly different red or blue, brought it out; (2) the same is true of a light green and light yellow; (3) in many cases while not succeeding in obtaining colors that would cause the illusion to disappear, we succeeded in finding for any given color a second with which the illusion is faint, and (4) we can effect this more systematically by combining with one of a pair of colors yielding the illusion sufficient white or black to cause it to vanish. In a vague and popular sense we call a given red lighter or darker than a given blue, but the physicist (as we understand it) has no accurate determination of this impressionist estimate; perhaps for ordinary empirical purposes it would be of advantage to call two colors equally dark when they fail to give the illusion now under discussion.

There is a factor in the illusion not yet considered, viz., the color of the interrupting rod. Heretofore this has been a copper wire; and whenever the illusion is distinct the color of the wire is of very slight importance, but when it becomes difficult to observe, then wires of certain colors will produce it and of others will not. The general outcome of much experimentation with colors hardly sufficiently contrasted in shade to produce the illusion is this, that with the component colors both rather dark, whether in proportions giving a light band or a dark one, dark wires will produce it, but light ones will not, with the component colors both light, light wires will produce it but dark ones not.[1] We are unable to bring this result into harmony with the ordinary laws of contrast, and must be content to give the empirical result without explanation.

We have but one further mode of observation that sheds light on the present point. We can obtain the illusion quite as well by substituting for the disc a cylinder covered by a strip of colored paper with a small strip of another color crossing it. We happened to use a rubber band to hold the second strip in place and noticed a deep contrast band parallel to the rubber when in rotation. We substituted a lead-pencil mark for the rubber and still obtained the deep band, *this band being of the same color as the bands* produced by passing a rod before the disc or cylinder. A lead-pencil mark on the disc will have the same effect. We observed however that this appeared only when the line passed across the color present in lesser proportion, which at once suggested (conformably to the experiment with the open disc) that the bands are originated during our vision of the minority color. We tested this by fixing a strip of brass to the disc in such a way that it could be made to rotate on its own centre (by striking against a fixed point) during the rotation of the disc. This device replaced the rod and caused the illusion so long as it was fixed to the minority color but not when fixed to the majority color. This offers some clue to the kind of contrast involved in the illusion but still leaves room for a satisfactory explanation.

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[1] The different colors of wire were simply insulated wires with the colors of the insulation different.

The chief points of our study may be thus resumed:

(1.) The illusion appears whenever the component colors are moderately contrasted in shade, and the one is present in distinctly greater proportion than the other; a difference in color, but not in shade, does not produce the illusion.

(2.) For all purposes affecting the illusion (except certain points of color) alternate appearances of an object against different back grounds is equivalent to alternate appearance and disappearance of the object.

(3.) The fact that several bands are seen is due to the persistence of the after image.

(4.) The distance between the bands is the distance through which the rod has passed in one revolution of the disc.

(5.) With the majority color darker than the minority color the bands are darker than the resulting mixture, and lighter when the majorily color is the lighter.

(6.) The width and rate of movement of the rod as well as the rotation-rate of the disc determine the width of the band; the color of the rod becomes important when the illusion is difficult to obtain, it then appearing that with the dark colors a dark rod is better, with light colors a light rod.

(7.) The bands originate during the vision of the minority color.

(8.) The contrast effect of the bands (while not satisfactorily explained) may also be obtained by a mark upon the minority color.

ACCESSORY APPARATUS FOR ACCURATE TIME-MEASUREMENTS.

(With the Assistance of FREDERICK WHITTON.)

A large portion of the measurement of the times of mental processes has been accomplished with the aid of the Hipp chronoscope; the objections to the use of this apparatus are the difficulty of its regulation and the possible sacrifice of accuracy to convenience. To secure accuracy, the chronoscope must be constantly controlled, and for this purpose complex devices have been resorted to for ensuring constancy of conditions and the like.[1] To simplify the method of control we succeeded after many trials and failures in perfecting a piece of apparatus by which the error of the chronoscope could be readily and accurately determined. In all methods in which the control consists in timing the fall of a ball (and this too is our method), the general problem is this: two pairs of events are to occur simultaneously, first the ball to begin its fall and the hands of the chronoscope to move, and then at a definitely measurable point of the fall, the hands of the chronoscope are to stop. The difference between the present and all other methods of securing this end (and to which we think its success is due) is that this simultaneity is obtained not by allowing a magnet to release a ball and also a blade held against the action of the spring or the like, but by the use of a special form of key. A simple movement of this key serves to *make* one of two independent circuits and to break the other at the same moment. The explanation of this key will be easier if preceded by a description of the form of key used almost exclusively in this laboratory for finger reactions. It is a key that when once closed remains closed and when opened remains open; this gives the advantage of having the closing and opening movements the same, and allows this movement to be the very natural one of quickly tapping the key and then withdrawing the hand. A brass arm CC' pivoted at its centre upon a brass upright terminates above at each end in a hard rubber button BB', and below in a brass point XX'; projecting from the board upon which the whole is mounted are two brass points YY' for the purpose of making or breaking contact with XX'; finally there is fastened to the arm CC' a wedge-shaped piece of brass playing between the notches 1, 2. The key as pictured is ready to be used to break a circuit, made through the point X' connected with the binding-post P', and the point central support connected through the apparatus with the binding-post P. A simple pressure of the finger at B' breaks the contact at X' Y', and forces the wedge into the position (2), in which it is securely held by the notch (2). When in this position it is ready to be used to *make* a circuit by a pressure upon B'; it can only assume one or the other of these two positions, and in either case is securely held in place. Now imagine that the button B' instead of being of hard rubber is of brass, and imagine the end of a second brass lever at right angles to CC' in position to press down upon B' and thus establish a circuit between B' and the second lever; imagine further that the blow of this second lever upon B' is given by the release of a strong spring that holds everything firmly until X comes in contact with Y, and the apparatus is complete. A release of the spring thus establishes one circuit through B' and the second lever which sets the chronoscope going, and at the same moment the ball begins to fall by the breaking of the circuit at X' Y'. The circuits are entirely independent and supplied by different batteries. To test the simultaneity we make our connections so that the making of the one circuit sets the chronoscope going and the breaking of the other stops it; and in no case did the chronoscope hands show the slightest tendency to move.

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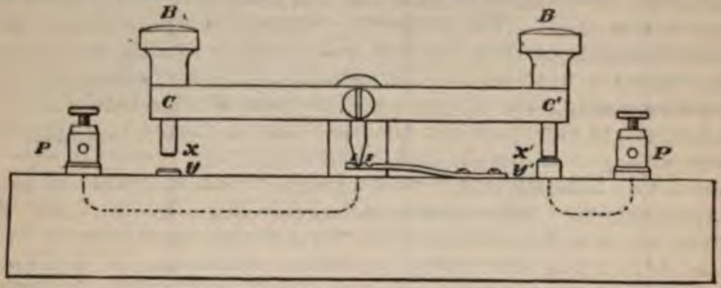
[1] The apparatus supplied with the chronoscope is altogether defective, the mechanical release of the ball is bad, the measurement of the falling height uncertain, the catch by which the circuit is closed imperfect and slow. In addition to these mechanical defects, the range of height is much too meagre. See Wundt's note to the same effect. Phys. Psych., 3d ed., II. p. 276. Note.



The apparatus controlling the fall of the ball is simple. An electro-magnet tapering to a point at one end is tightly held in a bracket, adjustable along a vertical slide, which in turn is securely fastened to the window frame. It is important that all parts of this be strong and securely fixed to the wall of the building. The slide is 6 to 7 feet high so that a fall of .6 to .7 seconds can be measured. From the value of g at Madison we calculate from the formula $s=\frac{1}{2} g t^2$ the heights at which the ball should just consume .1, .2, .3, .4, .5 and .6 seconds in its fall and mark these points on the millimeter scale along the slide, making our readings by aid of a fine wire. The ball of soft iron about ¾ of an inch in diameter is held at the tip of the magnet and in its fall strikes against the arm of a well-balanced lever, and thus severs an electrical connection by which the clock comes to a standstill; while the distance between the upper surface of the lever and the lower surface of the ball is the space fallen through in the measured time. Two further points may be noted; first to find the zero point on the scale let the magnet hold the ball and move the bracket down until the ball just touches the lever sufficiently to break the connection, and mark the point opposite the wire zero; second, use three or four thicknesses of tissue-paper between the ball and the magnet to separate the surfaces and thus diminish the time of demagnetization. With this apparatus one can without assistance take half a dozen records at different points in as many minutes; and in the work described above ten observations were recorded before and after each day's work, from which the error of the chronoscope for the day was calculated. As the observations were taken from all six positions—.1 to .6 sec. (in the latter portion of the work for four positions) we could determine whether the error was constant or relative and found the former to be the case. Throughout a period of six months, during which the chronoscope was tested, its maximum error was .005 seconds and the average error about .002 seconds. The position of the springs regulating the chronoscope was always noted and by changing these the error could be reduced to practically zero. But we aimed not at absolute accuracy but at an accurate determination of our daily error. This apparatus has proved itself so easy of manipulation and so time-saving, that its use is confidently recommended to experimental psychologists.

Note A.—On the Timing of Rotating Discs. A simple and fairly accurate means of determining the rate of rotating discs, especially of those rotating by clockwork, has long been a desideratum. The ordinary speed-counter is out of the question on account of the great friction involved. The "interruption-counter" invented by Dr. Ewald of Strassburg is a device by which each making of an electrical circuit moves the hand of a dial just one division, the dial showing 100 divisions; its original purpose was to count the vibrations of a tuning fork and thus to serve as a convenient form of chronoscope. It is capable of counting the vibrations of a fork with a vibration-rate of 100 per second, but for this, great delicacy of manipulation is necessary. Its adaptation to the present purpose is simple, though quite a number of devices were attempted before the simple one was obtained. Two small platinized tips were soldered at opposite points on to the circumference of the wheel of the clockwork next to the one to the axle of which the disc is attached. A light brass blade, also platinized, is suspended from above with a thumb-screw regulation, so that the tips on the wheel just make a coutact with it as they pass it. As this second wheel revolves once to every eight rotations of the disc we can count to the nearest four rotations, which is quite accurate enough. By increasing the tips we can count every two or every rotation, though the adjustment must then be finer. We allow the current to run through the counter for 15 seconds (as counted by the second hand of a watch) by closing and releasing a mercury key. We also devised a method by which the timing was done automatically and so one person could observe the disc and take the time measurements as well. This consisted in fastening to the ends of an ordinary revolving drum a circular piece of paper with a strip extending over about 180° cut out; by placing the end of a fine wire opposite this paper it is easy to arrange one's circuits so that during the time the wire touches the brass of the drum the counter is recorded, while for the rest of the revolution of the drum the current is intercepted by the paper; finally we set the drum so that the time of contact is a convenient one, say 15 sec., and when we see the contact approaching close, we lock the key and go on with our observation. The counter then of itself begins to record, does so for exactly fifteen seconds and stops; and we can make the reading at our leisure. For all these purposes the counter proved itself an exceedingly valuable apparatus.

As this is one of the first of these instruments to be used, our experience with it may be of advantage to others. Its two defects are that the wire on the magnets is too fine, thus causing an excessive resistance, and that the spring by which the magnet blade is withdrawn is not adjustable. After remedying these defects we were able to successfully manipulate the instrument with a single storage cell battery and very little trouble. We also tested the apparatus with a tuning fork of 100 per second and found it reliable. If the instrument were made as large again its efficiency would be increased.

Note on a device for color mixing. One objection to the ordinary method of mixing colors by forming sectors of them upon a disc and rapidly rotating it, is that while the mixture is produced one cannot readily compare the result with the original component colors. It is as a corrective of this defect that the following device is suggested. It consists simply of using a half disc (or any other desired portion most easily obtained by sliding two half discs or four quarter discs upon one another) of the one color and during its revolution holding the other color in back of it to one side. Then on either side you have the original colors, and where the two overlap the resulting color; if the colors be red and blue, you have before you on either side the red and blue and between them a purple. One can hold two (and with proper arrangements more) different colors in back of the same rotating disc and thus show for instance the mixture of blue with red and blue with yellow and the original blue, red and yellow all clearly displayed in line. One can show the mixture of an entire series of colors with the same color without stopping the disc, and for matching a given color with a resulting mixture this is especially convenient. With two rotating discs, overlapping upon a common background one can show the result of mixing three colors and the three original colors at the same time, but there the manipulation is no longer so simple. The method is easily adapted to the fusion of other visual impressions and is particularly suited to class-room demonstration. A clockwork for rotating the disc is a great convenience in the experiment.

THE PSYCHOPHYSIC SERIES AND THE TIME-SENSE.

(With the assistance of W. B. CAIRNES.)

In an earlier paper reporting the studies made in this laboratory (AMER. JOURNAL OF PSYCH., Vol III, No. 1, pp. 44-49) it was shown that when we attempt to sort out sizes of sticks into six or nine magnitudes, either by the eye or by passing the finger over the sticks, the result is that the average lengths of the sticks of the several magnitudes are separated by approximately equal differences; i. e., they form an arithmetical series. This method was spoken of as that "of the psychophysic series," and consists simply in distributing according to a general impression a large variety of sense-impressions into classes or magnitudes; it is also the method by which the stars were divided into their magnitudes. If the psychophysic law holds when thus tested the result would be, as it notably is, in case of the stars[1] that the ratios of the averages of neighboring magnitudes would be constant, i. e., would form a geometrical series. A suggestion of an explanation of the applicability of the law to star magnitudes and its failure in magnitudes of extension both visual and tactual-motor, was recorded in the former study in the following words: The law may be expected to apply to "such sensations as are appreciated *en masse*, and with not too distinct a consciousness of their intensity [or extension]; when the sensation is a sort of impressionist reception of the gross sensation without dividing it up into units, or conceiving it as so composed, we may expect the law to hold good. This would be the case with the rough estimations of star brightnesses." To further test this point of view we experimented with the perception of time-intervals, in which as in the estimation of star magnitudes there is an unanalyzed appreciation of the interval, without regarding it as composed of constituent units; and for which, according to the above suggestion, the law should hold good.

Accordingly we set a metronome at one of many intervals and asked the observer after he had listened to its beating as long as he desired in order to determine his judgment, in which of *six* classes of intervals he would place it. At the outset the observer was allowed to listen to the slowest interval, 40 per minute, and to the fastest, 208 per minute, and to imagine this range divided up into six grades or magnitudes. At first the assignments are somewhat vague and variable but they soon became relatively fixed, though there is considerable overlapping of the various magnitudes even in the best observers. Much of this is undoubledly due to contrast, an interval following a very long one seeming shorter than it would if following a short one, and the like. We used intervals rising by 2 per minute from 40 to 120 per minute, by 3 per minute from 120 to 144, and by 4 per minute from 144 to 208, thus using in all 63 intervals. These were written on small square cards and three sets of such, or 189 cards, were used at one sitting with each observer, the cards being tossed in a box and drawn at random, and the metronome set according to the number drawn. The longest time intervals, i.e., from 1.5 seconds down were called magnitude I, and the shortest from .29 seconds up, magnitude VI; the observer sat with his back to the metronome, knew nothing of the experiment except what were the longest and the shortest intervals, and simply called out the number of the class to which he assigned the given interval. Three such full sets of nearly 200 observations were made on one observer, two each upon two others, and one each on three others, making ten in all. When the results are obtained we collect all the intervals assigned to each of the magnitudes and find the average duration of the magnitudes of that interval, which averages will serve as the basis for the present discussion.

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[1] See the proof of this in an article Vol. I, No. I, p. 112 of this JOURNAL.

In the accompanying table are shown for each set of observations the average number of beats per minute of each magnitude, with the number of observations contributing to that average following it in small figures the successive differences and ratios of these magnitudes, and the average of these differences and ratios together with the average deviation from them expressed in percentages. At the foot of the table a similar series of weighted averages (i. e., results of multiplying each average by the number of observations and dividing by the total number of observations), is given, combining all the observations, and this we shall first consider. To test whether the series approaches an arithmetical or a geometrical series, we have simply to compare the *constancy of the differences with that of the ratios.* This may be done with sufficient accuracy for the present purpose by finding for each of the five results the differences from their average, dividing this by five, and expressing it as a percentage of the average of the five differences, or the five ratios. We thus see that while *the average variation from a constant difference is 24.8 per cent., the average variation from a constant ratio is only 4.2 per cent., indicating a decided approximation to a*

Magnitudes.		I.	II.	III.	IV.	V.	VI.	Aver-age.	Average Deviation	Ratio.
I.	W.B.C.	50.1^{29}	73.0^{29}	94.6^{30}	112.5^{34}	142.2^{28}	181.8^{37}			
	Differences, Ratios,		22.9 1.437	21.6 1.299	17.9 1.190	29.7 1.264	49.6 1.330	28·3 1·304	31·9% 4·9%	1 : 6.51
II.	W.B.C.	44.5^{23}	67.5^{29}	95.1^{44}	119.0^{35}	151.0^{24}	185.2^{22}			
	Differences, Ratios,		23.0 1.501	27.6 1.423	23.9 1.251	32.0 1.274	34.1 1.226	28·1 1·335	13·0% 7·6%	1 : 1.71
III.	W.B.C.	48.1^{21}	67.9^{29}	96.0^{47}	115.9^{42}	160.8^{25}	190.8^{22}			
	Differences, Ratios,		19.8 1.412	28.1 1.414	19.9 1.207	44.9 1.388	30.0 1.187	28·5 1·322	24·0% 7·5%	1 : 3.20
IV.	J. J.	48.0^{21}	64.1^{26}	89.2^{33}	106.4^{35}	140.5^{39}	186.1^{33}			
	Differences, Ratios,		16.1 1.335	25.2 1.392	17.2 1.193	34.1 1.320	45.6 1.325	27·6 1·313	35·4% 3·7%	1 : 9.57
V.	J. J.	50.6^{35}	77.7^{31}	97.6^{30}	118.5^{30}	146.0^{26}	184.1^{36}			
	Differences, Ratios,		27.1 1.535	19.9 1.256	20.9 1.214	27.5 1.232	38.1 1.261	26·7 1·300	18·8% 7·3%	1 : 2.57
VI.	R.H.T.	44.2^{9}	57.9^{22}	74.0^{27}	92.2^{44}	131.4^{42}	176.3^{44}			
	Differences, Ratios,		13.7 1.310	16.1 1.278	18.2 1.246	39.2 1.425	44.9 1.342	26·4 1·320	48·1% 3·8%	1 : 12.66
VII.	R.H.T.	44.4^{10}	54.0^{17}	81.8^{44}	105.0^{44}	132.7^{33}	178.9^{39}			
	Differences, Ratios,		9.6 1.515	27.8 1.216	23.2 1.284	27.7 1.264	46.2 1.349	26·9 1·326	31·2% 6·6%	1 : 4.73
VIII.	F. St.W.	48.1^{20}	69.4^{39}	98.0^{34}	123.6^{37}	157.6^{36}	183.1^{23}			
	Differences, Ratios,		21.3 1.443	29.6 1.427	25.6 1.261	33.4 1.270	26.1 1.166	27·2 1·313	12·6% 7·4%	1 : 1.70
IX.	S.S.B.	49.1^{34}	69.0^{31}	95.3^{46}	122.6^{35}	158.7^{28}	193.0^{23}			
	Differences, Ratios,		19.9 1.405	26.3 1.381	27.3 1.286	36.1 1.294	34.3 1.216	28·8 1·316	17·8% 4·6%	1 : 3.87
X.	S.D.J.	49.8^{29}	77.1^{33}	98.2^{35}	120.3^{40}	153.7^{22}	188.9^{29}			
	Differences, Ratios,		27.3 1.548	21.1 1.274	22.1 1.225	33.4 1.277	35.2 1.223	27·8 1·308	18·6% 7·2%	1 : 2.58
Weighted Average.	Weighted Average.	$50{\cdot}0^{23 \cdot 1}$	$69{\cdot}6^{28 \cdot 6}$	$93{\cdot}6^{37 \cdot 0}$	$114{\cdot}3^{37 \cdot 6}$	$147{\cdot}7^{30 \cdot 6}$	$185{\cdot}1^{31 \cdot 8}$			
	Differences, Ratios,		19·6 1·392	24·0 1·345	20·7 1·221	33·4 1·292	37·4 1·253	27·0 1·301	24·8% 4·2%	1 : 5.90

geometrical series, and therefore, according to expectation, an obedience to the psychophysic law. In the last column of all, the ratios of each pair of average deviations are given, and for the general result (accepting this rough mode of comparison), we have this, namely, that the approximation is six times (5.90) as close to a geometric as to an arithmetic series.

We may instructively note too a few peculiarities of these results; first, that while the ratios of neighboring magnitudes are approximately constant, there is a tendency for these ratios to decrease slightly from I to VI, or to increase in passing from short intervals to long ones. A precisely similar result is found in the case of star-magnitudes; and in the latter case the observations are sufficiently extended and regular to warrant an emperical formula expressing the rate of increase of this ratio, with an increase in brightness of the star-magnitudes. Moreover, two further irregularities recorded in the study of star-magnitudes reappear in the present study. The first is that at one extreme the ratio tends to be unusually large, and at the other unusually small. This is due to the limitations of the series, and the fact that were there another magnitude at each end of the series, some intervals now placed in I or VI would be placed in the class below I, or in that above VI. The errors thus induced are evidently opposite in direction. The tendency is more marked in the star observations than here, but if we note the individual results we see that in seven of ten cases the ratio of I to II is markedly larger than the others, and in five cases the ratio of V to VI is appreciably smaller than the others. These peculiarities are good evidence of the similarity of the psychological processes employed in sorting stars and in classifying time-intervals with magnitudes. A marked peculiarity of the present series (and one that interferes seriously with its regularity), is the tendency to make only a slight division between intervals assigned to III and those assigned to IV, but a marked one between those assigned to IV and those to V. This tendency is present in nine of the ten sets, and is marked in six, and so can hardly be accidental. It seems to depend upon a habit of viewing III and IV as medium intervals, while V is already a short interval. A closely similar irregularity was found in the estimations of the star-magnitudes of Ptolemy and Sufi.

Regarding the individual results we notice considerable irregularity, some individuals maintaining the law much more closely than others, as is observed most readily by a view of the last column of the table. That much of this irregularity is due to the paucity of observations is indicated by the fact that the average deviations in the combined sets I, II, III, of

W. B. C., IV, V, of J. J. and V, VI, of R. H. T., are smaller than the average of the group of three or of two sets. Thus for W. B. C.'s three sets the average variation from a constant ratio is only 4.8 per cent., in J. J.'s two sets 4.2 per cent., in R. H. T.'s two sets 3.3 per cent., while the ratios of the average variations from a constant difference and a constant ratio becomes as 1:5.54, 1:6.62 and 1:10.97. It should also be noted that the number of intervals assigned to each magnitude differs considerably. In the general average the deviation from the average of 31.3 for each magnitude is 13.4 per cent. III and IV have most intervals assigned to them (perhaps because many doubtful ones are naturally assigned to the medium magnitudes). I and II have fewest.

One further point may be mentioned as supporting the supposition that with a more conscious analysis of time-intervals, with the establishment of a habit of estimating time by seconds, the tendency to follow the geometrical series will be diminished. Thus it is quite noticeable that the first sets of all three observers who went through more than one set approach more closely to the psychophysic series than the later ones, the average deviations in the two cases being about as 4 to 7. Perhaps this is accidental, but it certainly suggests a departure from the impressionist method of estimating intervals with which we set out. Of the remaining three records VIII is unsatisfactory and was so noted at the time, while IX and X are records of observers accustomed to astronomical work, in which the second and half-second interval is important. The acquired habit of analyzing time intervals according to standard units may thus account for their slight tendency to follow the psychophysic series in their case.

The result of the present study thus goes to support the suggestion that when we estimate sensations roughly and on general impressions, without comparing them with standard units, we naturally, though unconsciously, make use of a geometrical series. We make relative distinctions rather than absolute ones, and this is the natural basis of the psychophysic law. While the process is a rough one, and is often accompanied by much hesitation and little confidence, the average results are fairly clear, and add one more to the many illustrations of the statistical regularity of apparently lawless and entirely unconscious mental tendencies.

THE PSYCHOPHYSIC SERIES AND THE MOTOR SENSE.

(With the assistance of AUGUSTA A. LEE (MRS. CHARLES GIDDINGS).

As a further application of the method of the psychophysic series we experimented with a form of movement in which with the forearm supported at the elbow as a pivot the hand moved laterally for practically any distance from 5 to 190 millimeters. The extent of the movement was limited and measured in the following way: The hand held a glass pencil and supported the same along a straight edge, the pencil furthermore moving in the ridges of very finely grooved glass. Over this glass was mounted a skeleton triangle about 6 inches across the base and 20 inches in altitude, and the whole moved in a slide to or away from the hand holding the pencil, such movement limiting the pencil to movements of varying length between the sides of the triangle and parallel to its base. A scale at the side indicated for each position of the triangle the distance moved over by the pencil. After allowing the subject to move over the shortest and the longest distances he was asked to mentally divide this range into six classes or magnitudes, and assign the various distances presented according to the perceptions gained through the sense of motion (sight was of course excluded), to the various magnitudes. Though the average lengths of these magnitudes present considerable irregularity, they very clearly show that they do not accord with the psycho-physic law and that they roughly approximate an arithmetical series. The averages themselves, together with the number of observations contributing to the average, are given in the following table:

	I.	II.	III.	IV.	V.	VI.
A. A. L. (1)	14.8 (47)	40.5 (45)	75.6 (28)	100.6 (18)	135.8 (21)	166.8 (20)
A. A. L. (2)	20.9 (42)	58.5 (34)	93.7 (26)	123.4 (15)	152.0 (12)	169.5 (13)
E.	13.5 (16)	36.6 (20)	70.7 (25)	110.7 (16)	134.8 (17)	168.6 (5)
H.	15.7 (27)	48.3 (28)	80.3 (28)	121.4 (18)	156.5 (13)	181.0 (8)
J. (1)	9.6 (22)	30.5 (44)	60.6 (45)	89.1 (33)	120.4 (24)	144.8 (39)
J. (2)	7.8 (13)	25.0 (25)	53.6 (30)	84.6 (25)	112.5 (13)	150.9 (34)

To show how far these results favor an arithmetical and how far a geometrical series it will perhaps be sufficient to state the average deviation from a constant difference and from a constant ratio of each of these series, expressed as a percentage of the average difference and the average ratio of neighboring pairs of results.

	L. (1)	L. (2)	E.	H.	J. (1)	J. (2)
Average deviation from a constant ratio—	29.8	30.7	31.1	32.5	34.9	33.7
" " " " " difference—	1.33	17.9	19.3	19.0	12.3	16.6

This shows about twice as close an approximation to an arithmetical as to a geometrical series. If however, we take the average of all six series of each magnitude we obtain a much more pronounced obedience to an arithmetical series; the successive differences become 26.2, 32.5, 32.6, 30.2, 28.3, and the average deviation of these from a constant ratio is but 8.6 per cent. of their average value. Finding the average deviation from a constant in all six series we find no such reduction. It is 30.9 per cent.

If we take into account the varying number of observations contributing to each average by weighting each difference with half the sum of the number of observations of the two averages, the difference of which is expressed, we obtain a still closer approximation to an arithmetical series. For the various series the average deviations from a constant difference then become in percentages:

L (1)	L (2)	E.	H.	J. (1)	J. (2)
10.8	18.1	19.0	13.1	12.3	17.4

and for the combined average of all only 6.3 per cent. It may be noted too that the combination of L's as of J's two sets of observations conform more closely to the arithmetical series than either one, and that the greatest deviations from the constant ratio are apt to occur in the two extremes of the series, when the shortest and when the longest lengths are concerned, the reason of which is obvious. Incomplete as these results are, they are perhaps sufficiently definite to suggest strongly the inapplicability of the physo-physic law (when thus tested), to spatial impressions gained by fore-arm movements. These movements are not altogether dissimilar to those made in running a finger along a stick (v. these studies in this JOURNAL, Vol. III, p. 47), and in both cases the judgment of length is rather conscious and referred more or less definitely to units, probably to notions gained through knowledge of feet and inches. They thus form an additional corroboration of the generalization that we conform to the requirements of the psycho-physic law in gross, *en-masse*, analyzed impressionist judgments, but not in precise detailed, analyzed and considerate judgments. The experiences of a civilized environment have transferred many forms of sense-judgment from the former to the latter class, among them spatial judgments both visual and motor. In these, absolute differences become of equal, and, at times, greater importance to us than relative ones.

The Interference of Mental Processes — A Preliminary Survey.

(With the Assistance of W. B. Cairnes.)

The general field with which the present study deals, (though in a somewhat eclectic and tentative manner), is the power of carrying on two mental processes at the same time. How far, we naturally ask, is this possible, how far economical? How shall we conceive this mental simultaneity, how cultivate and develop the power? We know that the shortening of mental processes brought about by practice is largely due to the power of doing two things at once, is an overlapping of mental processes; we know, too, that when processes become automatic they may accompany more deliberate and reasoned processes without interference; and we further recognize that certain processes directed to a common end are almost as easily performed together as separately. On the other hand we observe that states of extreme concentration are characterized by immobility, even by a slackening of the automatic functions; we observe the various kinds of disturbance all indicating the interference of two or more mental processes; and we appreciate the necessity of dividing our work into small parts so that they may be easily absorbed and not over-tax our powers. In entering upon this general problem, we at once encounter the difficulty of defining the mental unit; what is a mental process? In a certain sense we are always doing two things at once; the rhythmical functions of circulation and respiration go on while we work; we walk and think, we eat and talk, we write and listen at the same time. In every game of skill several senses act at once; the eye and hand, the ear and mouth, taste and smell act together and aid one another. On the other hand, however, in an intense attention to some fascinating event we stand motionless and almost stop breathing; many persons when thoroughly interested while talking upon the street involuntarily slacken their pace, or stop altogether; few of those who illustrate their remarks by off-hand sketches can talk and draw at the same time, and so on. Our general inquiry is "What processes hinder, what aid one another;" the present study makes no attempt to answer this most important query, but simply describes a few facts and suggestions relating to a very small and special portion of the general field.

We choose as the two types of process, (1) the performance of finger movements, involving rhythm and counting, and (2) of such processes as adding and reading under various conditions. The former were written (by the usual method of a system of Marcy tambours) upon a rotating cylinder, while for the latter we simply noted the time of a set task, performed as rapidly as possible. Our records are in no case very full, and the conclusions drawn are suggestive rather than final. We will consider first the effect upon the movement of an accompanying mental task.

The chief movements used were:

(1) A regular beating with the finger at any rate the subject chose; this we speak of as an *ad libitum* movement.

(2) A movement as rapid as possible and still regular; this is a maximum movement.

(3) Beating in groups of 2s, 3s, 4s, 5s or more.

(4) Beating in alternate groups of 3s and 2s, and 6s, 4s and 2s.

(5) Keeping time to a metronome at different rates, to an air hummed to oneself, etc.

The method by which the effect of mental tasks upon these movements was estimated was to compare the *ease*, the *regularity* and the *time* of these movements when accompanied and when unaccompanied by mental operations. Our results are not sufficiently numerous to show carefully all those effects (time, ease and regularity), but in general certain tendencies are evident. The ease is shown not alone by the feeling of difficulty, but as well by the presence of errors, varying in kind and degree; so, too, even when the rhythm is maintained, it may be more or less irregular, and in turn this irregularity manifests itself in a slowing of the movements. This slowing up is the natural accompaniment of difficult processes. It will thus be seen that these three indications are closely connected with one another, each being in a measure indicative of the others and all evidencing the same points. The "normals" or times of movements with no accompanying mental process are naturally variable. The records upon six days for J. J. of an *ad libitum* movement were 335σ, 320σ, 318σ, 518σ, 388σ, 424σ, 326σ, while, when several records were taken in the same day, the variations were much slighter in extent. The rate of maximum movements is much more constant, as the following records (of J. S.) show: 152, 163, 140, 148, 160, 164σ. For beating in groups of 5 the records (of J. J.) have the following times: 1837, 1966, 1801, 1734, 1471σ, and so on. These figures may perhaps suffice to illustrate the range of constancy of the phenomena in question.

Our first query will be: How far (neglecting for the moment the nature of the accompanying mental operation) will various movements be interfered with by the accompanying process? Our facts suggest the conclusion that the simpler movements are less interfered with than the more complex ones; the records of *ad libitum* movements show no appreciable difference when accompanied or when unaccompanied by other tasks; maximum movements are always somewhat slackened by the accompanying task; beating in groups of 2s, 3s, 4s or 5s become successively more and more interfered with by accompanying mental processes, such interference appearing not very much in a modification of time, but in the irregularity, the presence of errors (there being as a rule more beats in a group then there should be) and in the feeling of strain; in such movements as beating in groups of eleven, of alternate 3s and 2s or 6s, 4s and 2s, frequent failures set in, and when the result is fairly successful, the time is increased and the record more or less irregular. We are unable to range the various movements in their order of relative difficulty by the amount of interference, but the extremes are very markedly differentiated.

Our second query relates to the amount of interference of different mental tasks. Reading words in construction, reading words disconnected, reading numbers and adding numbers were the chief types of processes used; of these, reading words in sentences is by far the easiest task, all the others tending to make the subject have each beat coincide with a word or addition, and thus slowing the process. Furthermore, any of the movements involving counting, (particularly alternating 3s and 2s and the like) were more interfered with by adding than by reading. But the most striking difference depends upon the manner of going through the mental process, that is, whether the reading, etc., is done aloud or to oneself. In the former case the interference sets in much sooner and is much more serious than in the latter. Even quite simple movements are rendered irregular by reading or adding aloud; and such movements as beating in 3s and 2s or 6s, 4s and 2s were practically failures in such a case, though very successfully done with silent reading. An intermediate process of mumbling seemed to yield an intermediate degree of difficulty. The interference manifests itself clearly in an increased effort, a great irregularity and presence of errors, and a lengthening of the time of movement. Motor processes thus seem to interfere with motor ones, while refraining from movement during intellectual effort would be helpful. Passing now to the effect of an accompanying movement upon the time of such operations as reading sentences, words or numbers, adding (both aloud and to oneself); our data are meagre, but the following suggested inferences, together with the facts that suggest them, may be noted.

(1) The time needed to perform these mental processes is distinctly increased by such accompanying movements, the extent of the increase depending upon the complexity of the movement. (The general average of all the records (107) shows an increase of 4.28 seconds or 30.8 per cent.; J. J., 6.5 seconds or 26.5 per cent.; W. B. C., 6.02 seconds or 36.6 per cent.)

(2) Comparing the process of adding with that of reading, the former is the more complex, and seems to be more interfered with by the accompanying movements. (Comparable records are only about a half-dozen of J. J.'s in which the percentages of increase are about as 40 per cent. to 30 per cent.

(3) Reading and adding aloud are slightly more interfered with by the movements than the same processes performed to oneself. (In six dozen records of J. J., the percentages of in-

crease in the two cases are 31 per cent. and 24 per cent.; in W. B. C., the result is obscured by other factors.)

(4) Of the effect of different kinds of accompanying movements the following may be mentioned.

(a) If the movements are rhythmical beats arranged in groups, like a line of verse or a measure of music, the time increases with the number of beats in a group. For W. B. C., with groups of 2, 3, 4, 5, 6, the times of reading the same passages were 10.4, 11.0, 13.8, 14.0, 15.4 seconds. In one case groups of eleven were attempted with an increase above the normal of about 80 per cent. A similar result appears, too, in attempting to keep time to a beating metronome every 2d, 3d, 4th or 6th stroke of which is marked by a bell, with the accented syllable to coincide with the stroke of the bell.

(b) Simple regular beating, whether to the accompaniment of a metronome or without, can be done without increase of time for reading or adding; for J. J. this is true independently of the rate of the interval. Indeed there is some evidence that a maximum rate of beating also hurries up the mental process. The movements that retarded the processes most were beating in groups of eleven, making three beats of the right hand correspond to one of the left, and beating in groups formed by a six, a four and a two in turn.

(5) Reading disconnected words is more interfered with than reading words forming sense; part of which is due to the tendency of making each word correspond to the beat. While all these points require further corroboration, our results are sufficiently suggestive to evidence the promise of research in this direction. The next step would be to make a detailed study of a few types of interference and accumulate sufficient records to allow of quantitative expression: This it is hoped will be undertaken upon some future occasion.

THE SIZE OF SEVERAL CRANIAL NERVES IN MAN AS INDICATED BY THE AREAS OF THEIR CROSS-SECTIONS.

HENRY H. DONALDSON AND T. L. BOLTON.

(From the Neurological Laboratory of Clark University.)

On several of the cranial nerves of man we have measured the areas of cross-sections, taken at definite points, and sought by this means to get a numerical expression for the size of these nerves. The immediate reason for the investigation was the desire to compare with normal material the cranial nerves of the blind deaf-mute Laura Bridgman in order to determine in her case how far these nerves departed from the normal size. The relation of the size of the cranial nerves to the other structures with which they are associated is a matter of much interest, but one to which, at the moment, we have nothing to contribute.

Little importance seems to have been attached to the size of these nerves by those authors whom we have been able to consult. In general the text books have nothing to say on the subject. Schwalbe (1), v. Gudden (2), Salzer (3) and W. Krause (4) have measured the area of the cross-section of the optic nerve in man, for the most part near the bulb, and have obtained areas as small as 7.09 sq. mm. Obersteiner(5) gives the average area as about 9 sq. mm. Since, however, our sections and theirs were not made at similar points on the nerve, a detailed comparison is unnecessary. In addition to the Bridgman specimen the material employed consisted of seven male and three female encephala. A few brief statements will be necessary by way of comment upon the Table I. in which we embody our results.

Only the first, second, third and fourth nerves have been studied. The olfactory bulb was sectioned where it was thickest. The olfactory tract where it was thinnest. The optic nerves about 10 mm. from the chiasma. The oculomotor nerves about 10 mm. from their superficial origin and the trochleares at the point where they lie on the lateral aspect of the brain stem.

In forming the table the distinction between the nerves of the right and those of the left side is neglected, but the

GIVING AREAS OF THE CROSS-SECTIONS OF SEVE
THE NER

Catalog Number.	Nationality.	Age.	Sex.	Weight of Encephalon with Pia in Grammes.	Cause of Death.	Autopsy, hours after Death.	Nerve I. Bulb.	
15	American.	20	Male.	1520	. . . . . .	24	8.88	. .
9	. . . . .	57	"	1464	. . . . . .	30	5.78	.
10	. . . . .	35	"	1443	Diffuse Nephritis.	22	9.50	. .
13	Negro, American.	60	"	1432	. . . . . .	20	9 23	. .
12	. . . . .	35	"	1419	. . . . . .	28	7.75	. .
3	Irish.	39	"	1393	Tuberculosis	10	. . . . .	. .
2	. . . . .	45	"	1367	Aneurism of Aorta.	18	9.09	. .
11	. . . . .	40	Female.	1196	Phthisis.	10	. . . .	. .
1	Belgian.	45	"	1173	Intestinal Abcess.	18	7.65	. .
14	. . . . .	65	"	1040	. . . . . .	30	8.48	phy ion.
L. B.	American.	60	"	1204	Lobar Pneumonia.	8	(r)6.34	. .

vertical glass plate upon which tracing paper was fastened. The outline of the picture on the tracing paper was then followed with a hard pencil; the outline in all cases being taken inside of the epineurium. The amount of enlargement was usually 25 diameters; for the smallest nerves, in some cases, about double this enlargement was used. The amount of enlargement was determined by projecting a surface ruled in squares 0.5 mm. on each side. This surface had been previously tested and the ruling found to be accurate. The parts of the projecting apparatus were rigidly fixed and the enlargement tested both before and after each set of observations. The error here depends on the accuracy with which the outline can be followed with the pencil, and amounts at most to 1 or 2 per cent. To balance this error two tracings were made from each section. The tracings were next transferred to tin foil by laying the paper over the foil and following the outline on the paper with a fine but blunt metal point, thus impressing it on the foil. The piece of foil was then cut out and weighed. Its weight divided by the weight of 1 sq. cm. of foil gave the number of square centimeters contained in it and this in turn divided by the square of the number of diameters by which it had been enlarged, gave the area of the section in its original size. If for the moment we consider the tin foil to have a uniform thickness, then the first source of error is that of impressing the outline traced on the paper, upon the foil. Next is the error due to cutting out the piece of foil. The cutting was done with a small, thin and pointed scalpel. The errors here are small and may be considered as less than 1 per cent. To balance them as far as possible each outline on the paper was twice impressed on the foil. Since each section had been twice outlined and each of these outlines twice impressed on the foil, there were finally four pieces of foil representing each section. These at first were weighed separately, to give us a notion of the amount of variation, but later in the investigation they were weighed all together, and the average taken. The weighing was done upon chemical balances weighing to tenths of mgr., and no error of importance entered these. The further reduction was simply a matter of arithmetic.

To return to the foil which is an all important factor. That used consisted of a continuous roll one foot wide. To obtain samples from this an accurately made square brass frame, enclosing an area 3 cm. on each side, was laid on the foil and the enclosed area of foil cut out with the scalpel. The weight of one sq. cm., obtained by calculation from the weight of pieces containing 9 sq. cm., was found to range between .0619 + and .0674 + grms. The average of 54 samples of the foil showed the weight of 1 sq. cm. equal to .648 grms., which is very nearly the mean of the extremes just given. Careful testing showed that the two inches of foil on each edge of the roll gave the minimum weight, so that the greatest variation was in a line from side to side, across the roll. The difference in the extreme weight amounts to about 9% of the smaller figure—.0619 grms. This gives the impression of rather more irregularity in the foil than really occurred. If we take the 54 samples we find that 40% of them are within ± 1% of the average and that 80% are within ± 2% of the average. Since the pieces used as samples were taken, as a rule, closer to the foil representing the nerve than they were to one another and in many cases the sample was taken from within the foil representing the nerve, the amount of error introduced by the variations in the weight of the foil can be calculated as within 2%. It will thus be seen that the cumulative errors due to outlining, cutting and variations in foil might amount to 5% but the probability of their doing so in any single instance was small.

In carrying out these measurements the usual rules employed in psycho-physical work to avoid prejudicing the results were followed. The results therefore are naïve and such coincidences as occur are entirely unforced. If we knew that the section as prepared on the slide had the same area as in the natural state we might end our discussion here. Since, however, the area has been influenced by the treatment of the specimen we are compelled to give our methods in detail and estimate, as best we can, the amount of correction required.

The fresh nerves were all placed in a solution of 2½% bichromate of potash plus ⅙ its volume of 95% alcohol. In this they remained for three weeks. They were then washed for a day in water, put in 95% alcohol for 3 or 4 days and finally in 80% alcohol in which they were kept until imbedded. We have determined that the reaction to reagents of the nerve-tissues of the sheep is similar to that of man. To test then, in detail, the influence of this treatment we took similar nerves from the sheep and subjected them to like conditions. For this purpose six olfactory bulbs, six olfactory tracts, and three pairs of optic nerves from the sheep were weighed and the volume taken and then carried through the several solutions.

Thus they were prepared as the human nerves had been. Finally in 80% alcohol the volume for the olfactory bulbs was found to be 5.2% greater than in the fresh specimen, that for the olfactory tracts 8.8% greater, and that for the optics 2.6% greater. So far as we have observed the variation in volume is symmetrical for the olfactory bulbs and tracts but for the optic nerves it is not symmetrical.

In the first two instances the square of the cube root of the total enlargement will give as the area desired.

Hence for olfactory bulb,	area =101.7%
" " tract,	" =102.0%
" *optic nerves,	" =105.8%

That is, the areas of the bulb, tract and optic nerves are respectively 1.7%, 2% and 5.8% more than in the nerve in its natural state. The original observations are therefore to be corrected for this increase.

Specimens were imbedded in celloidin in the usual manner. By cutting a section before imbedding, then carrying the specimen through the process and cutting another section, it was found that imbedding in celloidin did not influence the area of the section. The sections were treated as follows: stained in a solution of acid fuchsin (acid fuchsin 1 grm., 95% alcohol 80 c.c., aqua. dist. 80 c.c.) for 2 or 3 minutes, washed in water, dehydrated in 95% alcohol, and then cleared either in oleum origanum cretici or Weigert's mixture —3 parts of Xylol plus 1 part of anhydrous carbolic acid— and mounted in Xylol Balsam. Following the sections step by step through this process by taking the outline after applying each reagent, it was found that the treatment produced no change in the area. Other sections from the same specimens were stained for 5 minutes with Delafield's hæmatoxylin diluted to one-third its strength, and then dehydrated and mounted in the manner above described. After the hæmatoxylin, stain, treatment with the Xylol-carbolic clearing solution caused a well-marked swelling of the section and so an increase of area. Sections thus treated were not used for measurement and we only mention this reaction as perhaps of interest in showing the inter-dependence of the various reagents used because when the hæmatoxylin was cleared by oil of origanum the area of the section remained unchanged.

From this it was plain that in order to reduce the sections to natural size they needed to be corrected only for the swelling which had taken place in hardening the specimens. For this correction the numbers above given were used.

The only exception to this general statement was in the case of the optic nerves of Laura Bridgman which were so poor in medullary substance that it seemed fair to suppose that they would swell but very slightly, if at all, in the process of hardening and therefore the numbers which appear in the table represent the actual size of the hardened sections. The

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*The method of obtaining the figure for the optic nerve will be explained in another paper.

other nerves, III and IV, in Table I, were corrected as though like the optic nerve in reaction.

It need only be added that the microtome used for making the sections permitted us to adjust the position of the object so that the sections could be cut at right angles to the axes of the nerves. Care was taken to that this was done with all possible accuracy.

A source of error exists in the determination of the part of the olfactory bulb which is thickest and the part of the tract which is thinnest, but since the pairs of sections from the same brain coincide fairly well it does not seem to us that our results are seriously affected by this disturbing factor.

We conclude therefore :

1. That the symmetrical nerves in normal brains tend to be alike.
2. That there may be great differences between individuals in the size of these nerves.
3. That the figures in the table represent within ±5% the areasof the several nerves reduced to their natural size.

IN LAURA BRIDGMAN.

4. That the olfactory bulbs and tracts are small.
5. That the optic nerves—especially the left optic,—are very small.
6. That the 3rd nerves are normal in size.

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VISUALIZATION AS A CHIEF SOURCE OF THE PSYCHOLOGY OF HOBBES, LOCKE, BERKELEY AND HUME.

By Alexander Fraser, B. A.

Thought has often been designated, by prominent philosophical critics, a kind of natural language; but that, like language, it varies with different classes of individuals, and to what extent this fact may be regarded as the source from which arises the great variety of philosophical theory which exists in the world, has as yet been barely noticed. Just as men of different nationalities speak in different verbal languages, so do different types of individuals think in different thought-languages and, just as in the case of verbal languages, each thought-language is made up from various different sources, but has one dominant, characteristic foundation. In one type the characteristic thought-stuff may be visual, in another auditory, in another motor, and another we might perhaps conceive with Jäger to be based on the sense of smell. On every such fundamental *sensational* thought-stuff there is built up a further web of verbal thought-stuff, which consists in trains of words, each of which in turn is the name, mark, or sign of the other, and which, very much like a series of algebraic symbols, must be regarded as unknown quantities until, translated from one to another, they at last receive their values in the fundamental thought-stuff. The ordinary man never for a moment suspects the peculiar mental language in which he thinks, but lets his thoughts, however inconsistent and absurd, flow on uncriticized. His fundamental mind-stuff lies hidden beneath a veil of words. But the philosopher strips off this veil and lays bare, though not for himself yet for the critical onlooker, the true foundation. The work of the philosopher, in the light of our analogy, may be said to be the endeavor to translate the algebraic exposition of his mental life into the terms of his fundamental thought-stuff. And if such is the nature of philosophy, each distinct doctrine must be determined by, and can best be studied by becoming familiar with that particular thought-language which characterizes the mental temperament of the philosopher who presents it.

Taking this analogy as a standpoint, the object of the present paper is to offer a description and estimation of the sensationalist psychology in its first presentation by Hobbes, its development by Locke and Berkeley, and its culmination in the scepticism of Hume; in which an attempt will be made (1) to maintain that the predominating element in the thought of these men was Visualization, and (2) on the basis of this fact to offer a new criticism of the psychology of Sensationalism.

Hobbes.—Hobbes is the true precursor of Sensationalism. The following is a short summary of his psychology by Ueberweg:

"All knowledge grows out of sensations. After sensation there remains behind the memory of it, which may reappear in consciousness. The memory of objects once perceived is aided and the communication of the same to others made possible by signs, which we connect with our mental representation of these objects; for this purpose words are especially useful. The same word serves as a sign for numerous similar objects, and thereby acquires that character of generality which belongs only to words, and never to things. It depends on us to decide what objects we will always designate by the same word; we announce our decision by means of the definition. All thinking is a combining and separating, and adding and sustracting of mental representations; to think is to reckon."

Knowledge according to Hobbes has two sides. (1) Knowledge of facts, on which side, is included (a) "sensations," (b) "images," "phantasms," "remembrances," "thoughts," by all of which he means the same thing, (c) trains of images or thoughts. (2) Knowledge of the relations of facts which he calls general knowledge or science. But this second side of knowledge is not recognized as a truly mental process. It belongs to that portion of the mental language which we designated "verbal-stuff." Though he recognizes it as the highest qualification of man, yet he cannot translate it into his sensational thought-language and hence cannot agree to call it a part of the mental process.

"For besides sense and thoughts and the train of thoughts, the mind of man has no other motion; though by the help of speech and method the same faculties may be improved to such a height, as to distinguish man from all other living creatures."

It is plain from the above passage that Hobbes' sensational thought-stuff consists of these "thoughts" or "images." These are the fundamental terms in which he conceives mind to think. And all we have to do now is to ascertain to what particular mental language they belong. To do this we need only ask Hobbes himself what he means by an "image."

"An image, in the most strict signification of the word, is the resemblance of something visible: in which sense the fantastical forms, apparitions, or seemings of visible bodies to the sight, are only *images;* such as are the show of a man, or other thing in the water, by reflection or refraction; or of the sun or stars by direct vision in the air; which are nothing real in the things seen, nor in the place where they seem to be; nor are their magnitudes and figures the same with that of the object; but changeable by the variation of the organs of sight, or by glasses, and are present often times in our imagination, and in our dreams, when the object is absent; or changed into other colors and shapes, as things that depend only upon the fancy. And these are the *images*, which are originally and most properly called *ideas*, and *idols* and derived from the language of the Grecians with whom the word Εἴδω signifieth *to see*. They also are called *phantasms*, which is in the same language, *apparitions*. And from these images it is, that one of the faculties of man's nature, is called the imagination. And from hence it is manifest, that there neither is, nor can be any image made of a thing invisible.

"It is also evident, that there can be no image of a thing infinite: for all the images, and phantasms that are made by the impression of things visible, are figured; but figure is a quantity every way determined, and therefore there can be no image of God; nor of the soul of man; nor of spirits; but only of bodies visible; that is, bodies that have light in themselves, or by such enlightened."[1]

From this it is quite evident that Hobbes identifies the whole process of imagination with that of visualization. Hear him again identifying the whole intelligible process with that of "seeing" in his interpretation of the doctrine of the "philosophy-schools."

"But the philosophy-schools . . . . teach another doctrine, and say, for the cause of vision, that the thing seen, sendeth forth on every side a *visible species*, in English, a *visible show*, *apparition*, or *aspect*, or *a being seen;* the receiving whereof into the eye, is *seeing*. And for the cause of *hearing*, that the thing heard, sendeth forth an *audible species*, that is an *audible aspect*, or *audible being seen;* which entering at the ear, maketh *hearing*. Nay, for the cause of *understanding* also, they say the thing understood, sendeth forth an *intelligible species*, that is, an *intelligible being seen;*

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[1] Leviathan, IV, 45.

which coming into the understanding makes us understand."

Here the only way in which he can understand the doctrine that a thing heard sends forth an audible species, is by viewing it as an "audible *being seen;*" and that a thing understood sends forth an intelligible species is by taking it to mean an "intelligible *being seen.*"

The truth of the matter is that Hobbes can hardly speak without betraying the fact that in so far as he is a psychologist he is a visualizer.

"No man therefore can conceive anything, but he must conceive it in some *place;* and indeed with some *determinate magnitude;* and which may be divided into parts."

Thus, I think, no further evidence is necessary to show that the sensationalism of Hobbes is strictly speaking only visualization.

Locke.—To Locke's philosophy, as in every other philosophy, there are two sides; there is the side which he worked out and explained and the side which he assumed but could not explain, the side which he faithfully deduced from his own original system, and the side which consists in fragments which he plucked from tradition to fill up the gaps in the former. The one is the sensational side, or rather that portion of knowledge which he succeeded in translating into his own particular kind of "sensational thought-stuff," and the other is that portion which he failed to translate and which remains in the form of "verbal thought-stuff," or untranslated "algebraic symbolism." The former is his celebrated theory of ideas and is the side which concerns us here.

According to this, all knowledge has its origin in sensation and reflection, the latter being considered as internal sensation. The endless variety and vast complex of human thought he scientifically reduces to its atoms, which he designates by the term "simple ideas." These are the "materials of our knowledge," and are "imprinted" on the senses whether we will or not. In this primary stage of knowledge the mind is "for the most part passive." Each simple idea is a distinct existence in itself, and is impressed on the mind as words are inscribed on a sheet of blank paper. There is no other source from which knowledge, however abstract and general, can come. But these ideas can be combined and permuted in an endless number of ways, which combinations are called "complex ideas." In these latter the simple ideas are not conceived of as mixed or blended, they are only conjoined; so that it is not necessary to decompose a complex idea in order to get its simple elements—it is only necessary to mechanically separate them. Consciousness, then, he makes to consist in a series of ideas which pass through the mind, one succeeding another "at a certain distance," somewhat after the fashion of a "train;" and from this fundamental view he goes on to show how the more complex and general forms of knowledge may arise. Now the question which demands attention is, What kind of sensationalism is it that is depicted here? What sort of sensational mind-stuff is at the bottom of such a theory?

The answer might be suspected at the outset if only Locke's method be observed. His method is that of introspection, and that in the strictest signification of the word. We find him constantly using such expressions as "if we look immediately into ourselves," "when the mind turns its view inwards," and many such terms all borrowed from the sense of sight. This at once suggests visualization. But let us see how he describes the results of his introspection. Speaking of the senses he says:

Locke II, 11:2.—"These alone * * are the *windows* by which *light* is let into this *dark* room; for methinks the understanding is not much unlike a closet wholly shut from light, with only some little opening left, to let in external *visible resemblances*, or ideas of things without."

In this passage the figure used to express the whole process of the understanding is taken from the sense of sight. The same kind of figure is used in his account of attention:

Locke II, 19:3.—"Sometimes the mind fixes itself with so much earnestness on the contemplation of some objects that it *turns their ideas on all sides*, *remarks* their relations and circumstances, and *views* every part so nicely, and with such intention, that it shuts out all other thoughts, and takes no notice of the ordinary impressions made then on the senses, which at another season would produce very sensible perceptions; at other times it barely *observes* the *train* of ideas that succeed in the understanding without directing and pursuing any of them; and at other times it lets them pass almost quite unregarded, as *faint shadows that make no impression*."

In his account of memory there is a remarkably strong tendency to visualization. He is constantly using such phrases as "ideas laid aside out of sight," "ideas lodged in the memory," "ideas imprinted on the memory," "dormant pictures;" and in one of the most eloquent passages of his book he describes the phenomena of forgetfulness thus:

Locke II, 10:5. "Thus the ideas, as well as children, of our youth often die before us, and our minds represent to us those tombs to which we are approaching, where though the brass and marble remain, yet the *inscriptions* are *effaced* by time, and the *imagery* moulders away. The *pictures drawn* in our minds are laid in *fading colors*, and, if not sometimes refreshed, *vanish* and *disappear*."

In the above passage we can see his description of consciousness as it is just going out—as it is becoming "not-consciousness." And what does it amount to? Nothing more than a waning or fading of visual images. The "pictures" fade gradually, and when they have so faded as to be no longer visible they are in the realms of the forgotten—they are no longer parts of consciousness. This view of consciousness is again brought out very decidedly in his distinction between "clear" and "obscure" ideas.

Locke II, 29:2.—"The perception of the mind being most aptly explained by words relating to the sight, we shall best understand what is meant by clear and obscure in our ideas by reflecting on what we call clear and obscure in the objects of sight. Light being that which discovers to us visible objects, we give the name of obscure to that which is not placed in a light sufficient to discover minutely to us the figure and colors which are observable in it, and which in a better light would be observable. In like manner our simple ideas are clear when they are such as the objects themselves from whence they were taken did or might in a well ordered sensation or perception present them. Whilst the memory retains them thus and can produce them to the mind whenever it has occasion to consider them, they are clear ideas.

"So far as they either want any of the original exactness, or have lost any of their first freshness and are, as it were, *faded* or tarnished by time, so far they are obscure."

The "clear" idea plays an important part in Locke's psychology. Though perhaps he is not fully aware of it, it must in reality be the criterion of knowledge, as is shown in the theory as developed by Berkeley.

Again it is evident that that *static* and *passive* character of the mind, which is so striking a feature of the sensationalist psychology, is chiefly due to the influence of visualization.

Locke Essay Bk. II, 1:25.—"No man can be wholly ignorant of what he does when he thinks. These simple ideas, when offered to the mind, the understanding can no more refuse to have, nor alter, when they are *imprinted*, nor *blot them out*, and make new ones itself, *than a mirror* can refuse, alter, or obliterate the *images* or *ideas* which the objects set before it do therein produce."

Locke does attribute an active character to the mind, but it is one of those processes he cannot explain—he has to leave it in the web of "verbal thought-stuff." When he attempts to explain the activity of mind in the moral world—he distinctly implies its deadness and passivity as *intellect*. He there maintains that the "*idea* of the greatest good" cannot determine the activity of the will, but the thing necessary to such determination is an "*uneasiness*"—the uneasiness of desire. He has to pass out from the sphere of dull, passive ideas or visual images and resort to other terms—"uneasiness."

Lastly, he speaks of consciousness as being a sort of "*train*" of ideas, each of which has a distinct existence in itself, and which, though separate and distinct, he assumes to have some sort of connection, but how or in what way he cannot discover. Now how does he come to have such a notion of the psychic process ?

Locke II, 14;9.—"I leave it to others to judge whether it be not probable that our ideas do, whilst we are awake, succeed one another in our minds *at certain distances, not much unlike the images in the inside of a lantern* turned round by the heat of a candle."

A visual figure again. I think nothing can be plainer than that this conception of thought as being a train of disconnected ideas, which have no connection in themselves but are tied together in some unknown way by some foreign tie, is just the outcome of the restriction of the whole psychic process to the partial process of visualization. In the process of vision one image comes and goes, another follows : we see both and can compare them ; they may be like or unlike, but in between them is a blank—we see nothing. The visual train is a broken train : it may be connected, indeed, but the connection is not of vision—it is a foreign element. And just such we have seen to be the case with Locke's "ideas." The ideas themselves present no difficulties, but the relations of ideas are the stumbling block—they cannot be visualized and hence there is a tendency to discard them. Locke tries hard to get an idea for "Substance"—tries hard to visualize it, but he cannot, and what is the result? "It is of no use in philosophy." Again he stumbles on the threshold of natural science. Natural science looks impossible to him. "There can be no science of bodies," and why? Because ". . . . . the simple ideas whereof our complex ideas of substances are made up, are for the most part, such as carry with them in their own nature no *visible* necessary connection."

Berkeley.—Berkeley is the first of the philosophers under consideration to state his doctrine in the terms of the analogy with which we started out. He represents the whole system of thought as a Universal Language.

"Hence it is evident that those things which, under the notion of a cause co-operating, or concurring to the production of effects, are altogether inexplicable, and run us into great absurdities, may be very naturally explained, and have a proper and obvious use assigned to them when they are considered only as marks or signs for our information. And it is the searching after and endeavoring to understand this Language of the Author of Nature that ought to be the employment of the natural philosopher; and not the pretending to explain things by corporeal causes, which doctrine seems to have too much estranged the minds of men from that Active Principle, that Supreme and wise Spirit "in whom we live, move and have our being."[1]

But though Berkeley's aim is to apply this Natural Language to the whole extent of thought, he has not, as we shall see later, succeeded. The language which he thus tried to apply was his own particular "thought-language" and was too narrow and limited to include all phases of thought. What this mental language is, is made very explicit in the first instalment of his philosophy, the "Essay towards a New Theory of Vision." It is the language of "visible ideas."

". . . visible ideas are the language whereby the Governing Spirit on whom we depend informs us what tangible ideas he is about to imprint upon us, in case we excite this or that motion in our bodies."

At first he *consciously* extends this visual language to the whole content of thought and explicitly asserts that it is the Universal Language of Nature.

"Upon the whole, I think we may fairly conclude that the proper objects of vision constitute the Universal Language of Nature, whereby we are instructed how to regulate our actions in order to attain those things that are necessary to the preservation and well being of our bodies, as also to avoid whatever may be hurtful and destructive of them. It is by their information that we are principally guided in all the transactions and concerns of life. And the manner wherein they signify and mark out unto us the objects which are at a distance is the same with that of languages and signs of human appointment; which do not suggest the thing signified by any likeness or identity of nature but only by an habitual connection that experience has made us to observe between them."[2]

But later on in his philosophy he recognizes the vast extent of thought and the inadequacy of his language, to cover it. He therefore seeks a wider language—the language, not of '*visible ideas*' but of "ideas." What he proved true of vision he seeks to show is true of the whole phenomenal world of sense. But in this he succeeded in doing little more than throwing a veil over his own eyes. What he did before knowingly and explicitly, he now does blindly and implicitly. Instead of broadening his language to suit knowledge, as he thought, he only narrowed knowledge to suit his language. His final Universal Language is nothing but the same old visual language, presented in faded colors. His theory of knowledge is easily recognized as a full acceptance and more thorough development of the visualization of Locke.

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[1] Berkeley Fr. Sel. p. 69.

[2] Berkeley, Theory of Vision.

"All our ideas, sensations, notions, or the things which we perceive. . . . . are VISIBLY INACTIVE—there is nothing of power or Agency included in them. So that one idea or object of thought cannot produce or make any alteration in in another."[1]

In this passage the static and inert character of the conscious process are forcibly insisted upon. The parts of thought are now strictly limited to the characteristics of visual images—they are "*visibly inactive.*" And the resulting philosophy that there is no *necessary* connection, no such thing as cause and effect, existing between ideas, is for the first time strongly emphasized. Ideas can *resemble* or be like or unlike one-another, just as visual images can, but just like visual images again there can be no passage from one to the other—the '*between*' is a blank; there can be no necessary connection, no cause and effect.

From this same pictorial way of thinking arises also the denial of the possibility of knowledge of any active being, principle or relations.

"A little attention will discover to us that the very being of an idea implies *passiveness* and *inertness* in it, in so much that it is impossible for an idea to do anything, or, strictly speaking, to be the cause of anything; neither can it be the *resemblance* or *pattern* of any active being."[2]

Berkeley indeed recognized the existence of active being and relations but they are things which he cannot explain—he cannot express them from his visual point of view. We have no ideas of such, we have only some vague, far off clue to their existence—they do not come to us with the warmth of ideas, we only may be said to have some "notion" of them.

"We may be said to have some knowledge or *notion* of our own minds, of spirits and active beings, whereof in a strict sense we have not ideas. In like manner we know and have a *notion* of relations between things and ideas— . . . . To me it seems that *ideas*, *spirits*, and *relations* are all in their respective kinds the object of human knowledge and subject of discourse . . . . . "[1]

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[1] Berkeley, Fr. Sel. p. 45.
[2] Berkeley, Fr. Sel. p. 45.

Lastly, the visual character of Berkeley's mind is brought out clearly in his violent reaction against abstract ideas. For him to abstract was an insuperable difficulty.

"For my own part, whenever I attempt to frame a *simple idea* of Time, abstracted from the succession of ideas in my mind, which flows uniformly and is participated by all beings, I am lost and embrangled in inextricable difficulties. I have no notion of it at all."[2]

We need only go to his own confessions, to be convinced of his peculiar mental temperament. "I can imagine a man with too heads," he says, "or the upper parts of a man joined to the body of a horse. I can consider the hand, the eye, the nose, each by itself abstracted or separated from the rest of the body. But then whatever hand or eye I imagine, it must have some particular shape and color."

Hume.—In Hume we have the visualization psychology presented in its purest and simplest form. What Hume cannot visualize he will not admit as belonging to thought or consciousness at all, but considers as "illusion." He reduces the whole world, material and mental, to "impressions and ideas," the only difference between which being not of kind but of "force and vivacity." There is no such thing as material substance because we can have no visual expression of it. Neither, for the same reason, is there spiritual substance, nor cause and effect, nor personal identity. Mankind he regards as "nothing but a bundle of different perceptions, which succeed each other with an inconceivable rapidity, and are in a perpetual flux and movement." Between these different perceptions there is no real connection nor continuity, no underlying substance nor cause and effect, which things cannot be *seen*, but their relations consist in "resemblance" and "contiguity" for the simple reason that these can be visualized. In his figure of the "theatre" the visual character of the mind is plainly enough depicted. Consciousness is a perpetual flux of totally different distinct and disconnected perceptions and nothing more. There are not even *vague* connections which can be properly called real parts of thoughts.

"The mind is a kind of *theatre*, where several perceptions successively make their appearance, pass, re-pass, glide away, and mingle in an infinite variety of postures and situations.

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[1] Berkeley, Prin. Hum. Kno. Sec. 89.
[2] Berkeley, Prin. Sec. 98.

"The comparison must not mislead us. They are the successive perceptions *only* that constitute the mind."[1]

What more beautiful figure of visualization could we ask for than this? When I introspect I am likened unto a spectator at a theatre, where I *see* the images pass and repass, etc.," all processes recognized by vision, but with this difference that I am, as it were, on the stage myself and consequently see nothing but the characters in the play, having no view of the stage on which they act.

Taking it for granted that we have already seen sufficient particular evidence of dominant visualization in each of our authors, let us now look for a moment at one point of more general evidence.

It was discovered by Galton in his thorough investigations into the faculty of visualization that in the case of children and young people the power is usually at its best, but that as years advance, at least in the case of those who are accustomed to hard abstract thinking, it undoubtedly becomes impaired, and, to a great extent, replaced by "verbal images." Now what application can we make of this fact to the case in question? In the first place we find that Hobbes, at the time he wrote his philosophy, was an old man of sixty-four. And we also find that in him visualization, though indeed the *only* sensational part of his philosophy, was a comparatively small one. It was only a small portion of thought that he succeeded in translating into his visual mind-stuff. The web of verbal images had become so firmly woven into his mind, that, though conscious of the importance of the task, he was unable to strip it off from any of the higher processes of reason and general knowledge. He was himself fully aware of holding such a position and gave it good expression in the following passage:

"A man that hath no use of speech at all, such as is born and remains perfectly deaf and dumb, if he set before his eyes a triangle, and by it two right angles, such as are the corners of a square figure, he may by meditation compare and find that the three angles of that triangle are equal to those two right angles that stand by it. But if another triangle be shown him, different in shape from the former, he cannot know without a new labor whether the three angles of that also be equal to the same. But he that hath the use of words, when he observes, that such equality was consequent, not to the length of the sides, nor to any other particular thing in his triangle, but only to this that the sides were straight and the angles three; and that that was all for which he named it a triangle, will boldly conclude universally that such equality of angles is in all triangles whatsoever; and register his invention in these general terms, *every triangle hath its three angles, equal to two right angles.*"

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[1] Hume, Treat I. 4:6.

Locke, too, was somewhat advanced in years when he presented his philosophical works—about the age of fifty-eight; and, as we have seen, his philosophy, like Hobbes', was under the necessity of leaving a great part of the verbal web untranslated. He tried hard to bring everything within the domain of vision, but he couldn't—his visual power being too dim, his verbal too strong.

In Berkeley and Hume we have the philosophy of youth. At the age of 25 both these men had completed their chief philosophical works. And, here again we have an illustration of Galton's results. Their powers of visualization were much higher than in the case of the former two men—so high, in fact, that they could visualize enough to make them believe that anything they couldn't visualize did not exist.

If what has already been said be true there must needs be a radical change made in the usual methods of criticizing the Humian psychology. In suggesting such a change I shall try to establish the following points: (a) The *method* of this school is right and its error consists in its incompleteness. (b) A wider sensationalism will overcome its difficulties.

(a) For our present purpose no better statement of the relative position of this psychology in the history of philosophy could be desired than that given by Professor James in that admirable chapter on "The Stream of Thought."

"If to hold fast and to observe the transitive parts of thought's stream be so hard, then the great blunder to which all schools are liable must be the failure to register them, and the undue emphasizing of the more substantive parts of the stream. . . . . Now such ignoring as this has historically worked in two ways. One set of thinkers have been led by it to *sensationalism*. Unable to lay their hands on any coarse feelings corresponding to the innumerable relations and forms of connection between the facts of the world, finding no *named* subjective modifications *mirroring* such relations, they have for the most part denied that feelings of relation exist, and many of them, like Hume, have gone so far as to deny the reality of most relations *out* of the mind as well as in it. Substantive psychoses, sensation and their copies and derivatives, juxtaposed like dominoes in a game, but really separate, everything else verbal illusion—such is the upshot of this view. The *Intellectualists*, on the other hand, unable to give up the reality of relations *extra mentem*, but equally unable to point to any distinct substantive feelings in which they were known, have made the same admission that the feelings do not exist. But they have drawn an opposite conclusion. The relations must be known, they say, in something that is no feeling, no mental modification, continuous and consubstantial with the subjective tissue, out of which sensations and other substantives are made. They are known, these relations, by something that lies on an entirely different plane, by an *actus purus* of thought, intellect or reason, all written with capitals and considered to mean something unalterably superior to any fact of sensibility whatever."

The criticism that is generally passed on the Humian psychology is that its very foundation is unsound—that its very method, that of sensationalism, must of necessity lead to scepticism, as is so excellently illustrated in the case of Hume. It begins, it is maintained, at the wrong end of knowledge. In order to explain knowledge we must not commence with sensation, but with thought, pure and undefiled by natural processes. Sensationalism, from its essential nature, must have "breaks"—it cannot supply the 'transitive" parts of consciousness. It can find a series of conscious states, but *only* a series. There can be no continuity running through them—there can be no connecting links between them. In order to such a continuity there must be an "*actus purus*" of thought. Now whatever be the faults of this method of psychology, it will become clear enough to any one who gives the matter fair consideration, that such a criticism and proposal of amendment can make it no better. Whatever be the value of pure thought in the wider domain of philosophy, for psychology it is not only useless, but nonsense. However pure and abstracted from feeling thought may appear to the disinterested onlooker, for the thinker himself it can never be present without some degree of warmth and feeling—it must always be present in terms of that same subjective mind-stuff of which our most familiar sensations and feelings are made up. So that if the Humian psychology fails to explain knowledge and leads to scepticism, it is not, at least from the psychological point of view, because it commences at the wrong end—not because its method and fundamental groundwork carry within their own nature the sceptical germ. Its aim and method is that of a complete sensationalism—that is, to make all parts of thought consist of the same continuous subjective thought-tissue; and this is the true method of psychology. The tendency to scepticism is not the outcome of this method—at least it has not yet been shown to be. Undoubtedly the rejoinder to this will be to point to Hume as a glaring practical illustration of scepticism being a consistent and the only consistent development of the sensationalist method. But this, I maintain, is unfair. The scepticism of Hume, as we have seen, is not the consistent outcome of *sensationalism*, but of *visualization*. It is not a philosophy resulting from being built on an unsound foundation, but from being built on one side only of a many sided foundation, and that only a particular and limited degree of that side. How a wider sensationalism both as an extension to the other senses and as a modification and more thorough development of visualization itself, may overcome many of the difficulties of Hume, will be suggested in our next point.

The psychological school which we have been considering is not only the outcome of visualization but of a particular degree of visualization. Galton in his experiments found that the degree in which this faculty exists in men is almost as varied as are the men themselves. Now if this be the case the philosophies resulting from visualization may be very different, and the faults and difficulties of one may be triumphed over in another, so that in this respect we can see the first possibility of a broader and more thorough development of sensationalism. To see the truth of this we need only resort to an illustration. Take for example the different interpretations of the concept or general ideas that have been given by visualizers:

The concept-theory with which the Humian psychology is identified is nominalism. According to this doctrine there is no general idea—the generality consists only in the name. The idea itself is some *distinct*, *particular* idea that has some time or other presented itself to the senses. It must have, Berkeley says, "some particular *shape* and *color*," and the only general quality which can be attributed to it is that it is "made to represent or stand for all other particular ideas of the same sort." This is a doctrine which results from one particular degree of visualizing power, but it is not the only one—there may be others.

In speaking of the visualizing faculty Galton says: "In the highest minds a descriptive word is sufficient to evoke crowds of shadowy associations, each striving to manifest itself. When they differ so much from one another as to be unfitted for combination into a single idea, there will be a conflict, each being prevented by the rest from obtaining sole possession of the field of consciousness. There could therefore be no definite imagery so long as the aggregate of all the pictures that the word suggested of objects presenting similar aspects, reduced to the same size, and accurately superposed, resulted in a blur. . . . ." If I mistake not, this resulting "blur" is very much like the concept described by certain upholders of conceptualism. Indeed, I think the word "blur" among the members of this school is quite currently considered a happy term. This, then, may be considered as another modification of a visual doctrine of concepts. But this is not all—there may be others still.

Huxley, speaking of the concept, says:

"This mental operation may be rendered comprehensible by considering what takes place in the formation of compound photographs—when the images of the faces of six sitters, for example, are each received on the same photographic plate, for a sixth of the time requisite to take one portrait. The final result is that all those points in which the six faces agree are brought out strongly, while all those in which they differ are left vague; and thus what may be termed a *generic* portrait of the six in contra-distinction to a *specific* portrait of any *one* is produced."

Here we have another phase of conceptualism brought to light through the scientific conception that generic images can be imprinted on the sight after the fashion of photography. In this case the generic character does not consist in the name, it is in the idea. Neither is the idea a "blur," it is clear and distinct. To what extent this degree of visualization exists in the world I cannot say, but there can be no doubt as to its possibility.

Besides this possibility of a broader psychology by means of variations in this one sense, there is a further possibility of the same, and on a more extensive scale, in the more harmonious development and co-operation of the other senses. Not only with such men as Hume, but with almost all men, there is a proneness to identify the whole sphere of consciousness with visualization. Our very language is a good index to this fact. When we wish to convey the idea that we understand, we invariably say that we "*see*." Again it is quite common and considered proper enough to speak of "degrees" of consciousness, some states being considered as quite "clear," others "fairly clear," and others "dim." If we have a "clear" idea of a thing we say that our consciousness of that thing is fully realized; if we have only a dim idea of it we say it is only partially realized, but that it is nevertheless all there in a potential state. Now, as is very clearly set forth by Professor James, an idea of an obscure or dim object is just as much consciousness as that of a clear one—the *consciousness*, if we are going to use the term at all, is just as "clear" in the one case as in the other. The truth is that the words "clear" and "obscure" are not properly applicable to consciousness as such. Again by a great many people the greater part of mental life—the passions, the sensations connected with the more unfamiliar senses, the motor sensations, the visceral sensations, and perhaps many sensations connected with hearing, are not recognized as consciousness at all, all of which are in reality as truly conscious activities as the clear and distinct phenomena of vision. In this we can see the foundation of that strangely contradictory doctrine of "*unconscious mental states.*" Many of our facts of consciousness come to us, as it were, already made up. We are left only the pleasure of analyzing them; all the nice rational synthetic work seems to have been performed by some other consciousness, or perhaps to a more physiological cast of mind it may seem to have been done by the nervous system. Such activities are *known* to be mental—they could not be otherwise, but still we know that "*we*" have not been conscious of them, and hence they have been called by such names as "latent reason" and "unconscious mental states." This condition of mankind seems very much like a normal hypnotic state in which all senses excepting sight are anaesthetic; in which they perform their work, not on purely mechanical principles, but in *secondary personalities* which do not participate in the primary visual consciousness. A good illustration of this is seen in some of M. Binet's Salpétrière subjects:

"Things placed in the hand were not felt, but *thought* of (apparently in visual terms), and in nowise referred by the subject to their starting point in the hand's sensation. A key, a knife, placed in the hand occasioned *ideas* of a key or a knife, but the hand felt nothing. Similarly the subject *thought* of the number 3, 6, etc., if the finger was bent three or six times by the operator, or if he stroked it three, six, etc., times.

"In certain individuals there was found a still odder phenomenon, which reminds one of that curious idiosyncrasy of 'colored hearing, of which a few cases have been lately described with great care by foreign writers. These individuals, namely, *saw* the impression received by the hand, but could not feel it; and the thing seen appeared by no means associated with the hand, but more like an independent vision, which usually interested and surprised the patient. Her hand being hidden by a screen, she was ordered to look at another screen and to tell of any visual image which might project itself thereon. Numbers would then come, corresponding to the number of times the insensible member was raised, touched, etc. Colored lines and figures would come, corresponding to similar ones traced on the palm; the hand itself or its fingers would come when manipulated, and finally objects would come, but on the hand itself nothing would ever be felt."[1]

It seems, just as in the cases quoted, that, the larger portions of our conscious life which we are liable to recognize as conscious are those which manage to translate themselves into visual terms; on which account the largest part of the content of consciousness is lost to view; all its finer connections and beautiful continuity remain, concealed in the anæsthetic senses, outside the primary consciousness, in regard to which they are blindly evolved and worked out by minor personalities.

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[1] James' Psychology, part I, p. 204.

That this is an injustice to consciousness, no proof is necessary. The remedy also is plain. It is obvious enough that what is needed for a more complete view of consciousness is a more equal emphasizing and more harmonious development of the senses. In support of the value of this suggestion I am not able to go very far. I shall only give an illustration which I hope will show the possibility of the method; and for this let us take the case of Hume, the arch-visualizer of our theme.

In the case of the passions Hume has a philosophy very different in many respects from that which he proposed for the intellect. Here he is not confronted with the difficulties with which he was surrounded in his theory of ideas—he meets with no isolated substantives which he cannot connect, but finds a beautiful continuity of consciousness; and though owing to his natural prejudice he is unable to recognize it as a process of consciousness, yet it must be considered of great value as an illustration of a more adequate view of thought being derived from other senses than that of sight. The sense he makes use of in the illustration which I refer to is that of hearing.

"Now if we regard the human mind, we shall find, that with regard to the passions, 'tis not of the nature of a wind instrument of music, which in running over all the notes immediately loses the sound after the breath ceases; but rather resembles a string-instrument where after each stroke the vibrations still retain some sound, which gradually and insensibly decays. . . . . each stroke will not produce a clear and distinct note of passion, but the one passion will always be mixed and confounded with the other. According as the probability inclines to good or evil, the passion of joy or sorrow predominates in the composition: because the nature of probability is to cast . . . . . a superior number of returns of one passion or since the dispersed passions are collected into one, a superior degree of that passion. That is, in other words, the grief and joy being intermingled with each-other, by means of the contrary views of the imagination, produce by their union the passions of hope and fear."[1]

To show how admirably this figure will allow of the proper unity and diversity of these passions I shall quote still further.

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[1] Hume Treat. II. 9.

"The passions of fear and hope may arise when the chances are equal on both sides, and no superiority can be discovered in the one above the other. Nay, in this situation the passions are rather the strongest, as the mind has then the least foundation to rest on and is tost with the greatest uncertainty. Throw in a superior degree of probability to the side of grief, you immediately see that passion diffuse itself over the composition, and tincture it with fear. Increase the probability, and by that means the grief, the fear prevails still more and more, till at last it runs insensibly, as the joy continually diminishes, into pure grief. After you have brought it to this situation diminish the grief after the same manner that you increased it; by diminishing the probability on that side and you'll see the passion clear every moment, till it changes insensibly into hope; which again runs, after the same manner, by slow degrees, into joy, as you increase that part of the composition by the increase of the probability. Are not these as plain proofs, that the passions of fear and hope are mixtures of grief and joy, as in optics 'tis a proof that a colored ray of the sun passing through a prism is a composition of two others, when as you diminish or increase the quantity of either, you find it prevail proportionably more or less in the composition?"[1]

As to the value of the illustration I shall leave it to the reader to decide. Yet I cannot refrain from remarking that in this there seems to be pictured a continuity of thought which cannot be conceived of through vision.

"In our present enthusiastic devotion to the eye it is not alone the symmetry of the mind that is threatened nor the voice arts alone that will suffer. It may be that we are neglecting that which is in itself one of the richest sources of good. It has not yet been shown that the world of form is more worthy of our cultivation than the world of sound. 'There is something as yet unanalysed about sound' says Mr. Haweis 'which doubles and intensifies at all points the sense of living: when we hear we are somehow more alive than when we see. Apart from sound, the outward world has a dream-like and unreal look—we only half believe in it; we miss at each moment what it contains. It presents, indeed, innumerable pictures of still life; but these refuse to yield up half their secrets."[2]

The starting-point of this paper was a suggestion by Dr. E. C. Sanford that I should investigate the figures of speech used in psychology. I am glad to express my indebtedness to Dr. Sanford both for this and for valuable direction in my investigations.

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[1] Hume Treat. II. 9.

[2] G. T. St. Patrick, Rivalry of the High Senses.

ANATOMICAL OBSERVATIONS ON THE BRAIN AND SEVERAL SENSE-ORGANS OF THE BLIND DEAF-MUTE, LAURA DEWEY BRIDGMAN.

HENRY H. DONALDSON, PH. D.

II.

I.—On the thickness and structure of the cerebral cortex.

PLATES III AND IV.

In a previous paper (AM. JOURN. OF PSYCHOLOGY, Vol. III, No. 3, Sept., 1890.) I have described some of the macroscopic features of the brain in question. I there stated the results of the measurements of the extent of the cortex (loc. cit. p. 336) as follows:

Extent of cortex, right hemisphere = 98946.5 □ mm.
Extent of cortex, left hemisphere = 101256.0 □ mm.
Total extent of cortex = 200202.5 □ mm.

It has been recognized by all those who have studied the extent of the the cortex, that unless supplemented by observations on the thickness and character of the same, the figures for extent did not give a good ground for further inference. Jensen(45) is, however, the only investigator who has up to this time made his studies thus complete.

It is, therefore, my purpose to report the results of the examination of the cortex of Laura Bridgman together with such conclusions as may be drawn from the results.

I.—The thickness of the cerebral cortex in general.

By way of preface I made a little excursion into the literature of the cortex to determine what was considered to be the normal thickness of that layer. It is highly probable that some of the work on this subject has escaped my notice, but what was found is tabulated (Table I.) with the purpose of showing how fully the various authors have stated the manner in which they obtained their results and what corrections had to be made, in certain cases, in order to have the results fairly comparable.

TABLE I.

Thickness of Cortex.

Date.	Authority.	Acquired defect.	No. of Brains Examined.	No. of localities in each hemisphere.	Special locality.	Average; localities not given.	Measuring Instruments.	Correction for Compasses.	Condition.	Correction for hardening.	Measurement taken in	Natural thickness of cortex in mm.	
1841	Parchappe(60)	. . . . .	. . .	.	Ant. lobes. Base & convexity	. .	. . . . . .	. . .	. . . .	. . .	. . . .	2.–3. 2.5–5.	Normal.
1841	Baillarger(54)	. . . . . .	. . .	.	. . . . . .	Average	. . . . . .	. . .	Fresh (?)	. . .	Paris lines	3.37	
1865	Engel(55)	. . . . . .	. . .	.	. . . . . .	“	. . . . . .	. . .	. . . .	. . .	. . . .	2.2–3.7	
1875	Jensen(45)	. . . . . .	4	18	. . . . . .	. .	Compasses	+ 4 %	80 % alcohol	+ 2 %	. . . .	2.91	
1878	Richet(56)	. . . . . .	. . .	.	. . . . . .	“	. . . . . .	. . .	Fresh(?)	. . .	. . . .	3.00	
1879	Bucknill and Tuke(57)	. . . . . .	. . .	.	. . . . . .	“	. . . . . .	. . .	. . . .	. . .	Inches	2.03	
1880	Danilewsky(49)	. . . . . .	. . .	.	. . . . . .	“	. . . . . .	. . .	Fresh	. . .	. . . .	2.50	
1884	Conti(58,59)	. . . . .	10 M.* 8 F.†	26	. . . . . .	. .	Compasses	+ 4 %	Fresh	. . .	. . . .	2.25 M. 2.24 F.	
1886	Franceschi(61)	. . . . .	10 M. 10 F.	35	. . . . .	. .	Compasses	+ 4 %	Fresh	. . .	. . . .	2.48 M. 2.46 F.	
1887	Luys(62)	. . . . . .	. . .	.	. . . . . .	Average	. . . . . .	. . .	. . . .	. . .	. . . .	3.–4.	
1888	Obersteiner(63)	. . . . . .	. . .		Gyr. Post. Centr. Occipital Pole.	.	. . . . . .	. . .	. . . .	. . .	. . . .	4. 1.5	
1891	Donaldson	. . . .	6 M. 3 F.	14	. . . . .	. .	Micrometer Eye Piece	. .	Bichromate and alcohol	— 2 %	. . . .	2.92 M. 2.91 F.	
1872	Major(64)	Insanity	4	30	. . . . . .	. .	Tephrylometer	. . .	80 % alcohol	. . .	Inches	2.37	Defective.
1875	Jensen(45)	“ “ Idiocy	2 M. 1 F. 2 F.	18	. . . . .	. .	Compasess	+ 4 %	Fresh	+ 2 %	. . . .	2.68 M. 2.79 F. 2.48 F.	
1879	Bucknill and Tuke(57)	Insanity	33 M. 30 F.		. . . . . .	Average	. . . . . .	. . .	. . . .	. . .	Inches	1.88 M. 1.85 F.	
1888	Cionini(65)	Gen'l Paralysis	8 M. 2 F.	31	. . . . .	. .	Compasses	+ 4 %	Fresh	. . .	. . . .	1.83 M. 1.84 F.	
1891	Donaldson	Arrested devel-opm't. L. B.	1	14	. . . . .	. .	Micrometer Eye Piece	. .	Bichromate and alcohol	— 2 %	. . . .	2.59	

*M.=Male
†F.=Female

The authorities are arranged in chronological order, and in two groups: the first group containing the figures which apply to the cortex of normal persons, and the second the figures that apply to defectives. In this latter group I have only the measurements that apply to individuals with an acquired defect, as contrasted with those congenitally defective. The literature bearing on the cortex in these last has been brought together by Marchand(66), and, though the facts are very interesting, they do not bear on our present problem and are therefore excluded.

The headings of the columns in Table I. will explain themselves, I trust, and the Table may be examined now without further explanation.

Omitting my own results, there are but six authors whose figures are of interest to us now. The manner in which the final figures in these cases have been obtained requires some explanation.

We desire to know the thickness of the cortex in its natural state, but the hardening reagents used for preserving the brain alter the thickness. In another place, I expect to make some general statements with regard to the weight and volume of nervous tissues as influenced by hardening reagents. Therefore I may state here only the results obtained, viz., that alcohol of 80% causes a decrease of 2% in the thickness of the cortex, while the bichromate and alcohol treatment (potassium bichromate 2½% plus ⅙ its volume of 95% alcohol for 6 to 8 weeks; washing in water for 24 hours; alcohol 95% for 2 days, and final preservation in 80% alcohol) causes an increase of 2%. As will be seen these corrections have been applied in Table I. Further, the manner of making the measurements has a very decided influence on the results. Direct experiment showed that the same localities measured with the compasses gave a thickness 4% less than when measured with a micrometer eye-piece under the microscope. There is no doubt in my mind that the microscopic method is the more accurate, hence I have corrected all the measurements made with compasses by the percentage above found.

There still remains the important question of the handling of the figures for thickness after they are obtained. In general, the summit of a gyrus has the thickest cortex and the very bottom of the sulcus, the thinnest. In getting the thickness for any locality on the hemispheres at least two measurements, a maximum and minimum, are taken. Most investigators have measured the gyri at the points where the very thickest and very thinnest cortex was to be found, and for an average taken half the sum of these figures. The thinning of the cortex at the bottom of the sulci is, so to speak, sudden and excessive and the thinnest point deviates more from the intermediate cortex than does the thickest. Such being the case the resultant figure is somewhat too small. Conti(59), Franceschi(61) and Cionini(65) give full tables and they have measured in the manner above described so that their averages represent one-half of the sum of the thickest and thinnest points in each gyrus. In the brains which I have examined the thickest portion was measured at the summit of the gyrus. The observations for the thinnest was taken at the side, about two-thirds of the distance from summit to sulcus. In making the average advantage was taken of the observation that one-third of the cortex lies at the summits of gyri and two-thirds is sunken in the sulci. The smaller figure was multiplied by 2, added to the larger figure and the sum divided by 3. As a consequence of this treatment I believe that my final average for the cortex of any particular gyrus is nearer the truth than it would be if half the sum of the thickest and thinnest points had alone been taken.

The figures which will be most useful to us can now be taken from Table I and presented in Table II, with the purpose of showing whether there is any difference in cortical thickness between males and females, or between the two hemispheres of the same brain; whether defectives correspond with normal persons; and what may be regarded as the normal thickness of the cortex.

Since the figures given in the Table II do not occur in their present form in the original tables of the authorities there quoted, I should perhaps add a word of explanation on the method by which they have been obtained.

Jensen(45) gives a condensed statement for the normal brains, and in Table II his figures are simply corrected for the effect of alcohol and the use of compasses in measuring. His tables for the defectives are fuller and permit us to determine the averages for the two hemispheres. These are corrected in the manner above mentioned. In no case did he measure the cortex of the insula. Among the defectives one case which he gives is not entered in the table because it is that of a microcephalic.

Bucknill and Tuke(57) give, without detail, the thickness of the normal cortex as .08 in. In a table of 63 pathological cases entered with great care and fullness, one column is devoted to the thickness of the cortex — also given without detail — in hundredths of an inch. This unit, approximately equal to .25 mm., is rather large when employed in so delicate a measurement. No statement as to the number, locality or method of their measurements is made. The cases were all adults.

Conti(60) gives full tables. He claims twenty brains in his series. The measurements on two brains — females — are, however, so incomplete that they are not used here, hence he is credited with but eighteen brains in the table. Both hemispheres were not always examined. The total number of hemispheres represented in the table is only twenty-nine, 16 right and 13 left. His cases, principally adults, range in age from sixteen months to eighty years, but there is no evidence that the youngest cases should be excluded. Twenty-six localities in each hemisphere were measured but the cortex for the insula, if measured, is not specially recorded. In the prerolandic and post-rolandic regions only the summits of the gyri and the depths of the sulci were measured. In the rolandic region intermediate measurements on each wall of the gyri were taken. The averages were obtained by summing and dividing the figures as they stand in his tables and then correcting the final results for the use of compasses. The original measurements were made in tenths of a millimeter.

Franceschi(61) gives full and very complete tables. He examined the cortex at 35 localities on both hemispheres of twenty brains, principally from adults of advanced age, 10 males and 10 females. The measurements taken in tenths of a millimeter, and were made at the summits of the gyri and the depths of sulci. The cortex of the insula was included. The figures in Table II. are obtained directly from those of his tables, save that they have been corrected for use of compasses.

Major(64) tested the thickness of the cortex at thirty localities on both hemispheres of the brains of four adult insane patients, the sex not given. For each locality he gives only the mean depth using one-fifth of an inch as his unit of measure. This unit is, of course, too large. He measured the insular cortex. His figures for the cortical thickness give the mean depth without detail as to the method of obtaining the mean. The instrument used, the tephrylometer, consisted of a thin walled graduated glass tube. This was pressed into the brain substance at any desired point, then, the upper end being closed by the finger, withdrawn, when a plug of brain substance remained within the tube and on this plug the thickness of the cortex is read off by the aid of the scale etched in the tube. The figures in Table. II are the simple averages of those in his tables without any corrections. Concerning the accuracy of this method of measuring the cortex there are no observations.

Cionini(65) presents his results from the examination of fifteen adult brains, ten males, five females, all cases of general paralysis. The number of localities was 31, but in other respects the details are similar to those in the case of Conti. It occurs, however, that in five cases, three males and two females, the tables are so incomplete that they cannot be used for averages, and hence only ten cases are represented. The figures in Table II. are obtained as in the case of Conti.

A glance at Table II. shows that in both normals and defectives the average thickness is very slightly, —.01 to —.04mm., greater in the males in five out of the six cases (larger number underlined). There is a slightly greater difference between the two hemispheres, which is in favor of the left hemisphere as the figures stand (eight out of thirteen cases). In discussing the absolute thickness of the cortex as reported we have, of course, to throw out the defectives, who are, *ipso facto*, expected to have a thinner cortex.

TABLE II.

Thickness of Cortex.

Authority	Males. No. of Brains	Males. Defect	Males. Right Hemisph'e	Males. Left Hemisph'e	Males. Average	Females. No. of Brains	Females. Defect	Females. Right Hemisph'e	Females. Left Hemisph'e	Females. Average	
Jensen[illegible],	4	. .	. . . .	. . . .	2.01	. . .	. . . .	. . . .	. . . .	. . . .	
[illegible] and [illegible],	. .	. . .	. . . .	. . . .	2.08	. . .	. . . .	. . . .	. . . .	. . . .	Normal.
[illegible],	10	. . . .	*2.29	2.21	†2.25	8	. . . .	2.24	2.25	2.24	
[illegible],	10	. .	2.470	2.474	2.48	10	. . . .	2.468	2.457	2.46	
Donaldson	6	. .	2.01	2.04	2.02	3	. . . .	2.00	2.02	2.01	
Major[illegible],	14	Insanity	2.368	2.370	2.37	. . .	. . . .	. . . .	. . . .	. . . .	
Jensen[illegible],	2	Insanity	2.66	2.68	2.66	1	Insanity	2.600	2.777	2.70	Defective.
	.	. . . .	. . . .	. . . .	. . . .	2	Insane Idiots	2.46	2.50	2.48	
[illegible] and [illegible],	38	Insanity	. . . .	. . . .	1.88	80	Insanity	. . . .	. . . .	1.85	
[illegible],	8	General Paralysis	1.800	1.851	1.83	2	General Paralysis	1.778	1.800	1.79	
Donaldson		.	. . . .	. . . .	. . . .	1	Arrested development	2.55	2.62	2.59	

* When there are averages for the two hemispheres the larger figure is doubly underlined.
† When there are averages for the two sexes the larger figure is underlined.
‡ Sex not given.

At the moment I have no explanation to offer of the various figures given for the absolute thickness in normal persons and will simply point out that my figures agree most closely with those of Jensen.

It appears, therefore, that the average thickness for the two sexes is nearly alike, what difference there is being in favor of the males; that the left hemisphere more often has the thicker cortex; that in defectives (not congenital) it is thinner than in normal persons, and that the figures given for the absolute thickness in normal persons are at present irreconcilable. With this I conclude the introductory study of the subject.

II. *Comparison of the cortex of Laura Bridgman with that of nine normal brains* (*six males; three females*).

The normal brains were obtained in New York about a year ago, and I am indebted to the courtesy of several medical gentlemen of the city for them. There is no reason to think that any of these specimens were from persons of more than average intelligence, hence on that score they are comparable with the Bridgman brain. They were hardened in the same manner that the latter was (*vide* p. 9). Samples of cortex were taken in all cases from 14 localities on each hemisphere, each locality being designated by an arbitrary number.

Plate III shows the localities with the numbers used, and is intended to take the place of a written description.

In Table III. I give the cortical areas in which the localities are situated.

All the samples from the several localities were treated in the same manner, viz.: imbedded in celloidin, cut in sections about 0.1 mm. thick and measured, unstained, under a low magnifying power. It is hardly necessary to add that all the

TABLE III.

Locality.	Cortical Area for.	Locality.	Cortical Area for.
1.	Speech motor ?	8.	Sight, sensory.
2.	Speech, motor.	9.	— ?
3.	Speech?	10.	Taste and smell, sensory.
4.	Head and eyes, motor.	11.	Sight, sensory.
5.	Arm, motor.	12.	Touch, sensory.
6.	Hearing, sensory.	13.	Leg, motor.
7.	— ?	14.	Sight, sensory.

measurements were concluded before any calculations were begun and that precaution was taken to keep the results unprejudiced.

Figures for the average thickness at each locality having been obtained from all the brains in the manner above described, the localities were arranged in order, from the thickest to the thinnest, and the tables thus formed were plotted as curves. *Vide* Plate IV.

The principal results are tabulated in Table II (under Donaldson, normals), and in Table IV a further analysis is given. The figures for males and females being separated in Table IV, those for the right and left hemispheres are given in each group and the individuals in each group are ranged according to age. This last arrangement was made to see whether they showed a decrease in cortical thickness with advancing age. Conti(58) reports that the cortex decreases regularly from a maximum at 3 years to a minimum in extreme age. I do not pretend to discuss the question here but simply refer to the table to show that these brains when thus arranged do not exhibit a decrease.

TABLE IV.

Thickness of Cortex in Controls and in Laura Bridgman.

MALES. Arranged according to age.				FEMALES. Arranged according to age.			
Age.	Weight in grms.	R. H.	L. H.	Age.	Weight in grms.	R. H.	L. H.
35	1419	[*] 2.81	2.81	40	1196	2.74	2.74
35	1443	2.87	3.09	45	1173	2.80	3.00
39	1393	2.77	2.86	Adult	1312	3.12	3.02
45	1367	2.90	2.93				
57	1464	2.96	2.91				
Adult	1210	3.14	3.07				
		2.91	2.94			2.89	2.92
General Average, 2.92.				General Average, 2.90.			
Laura Bridgman,				60	1204	2.55	2.62
				General Average, 2.59			

[*] The underlining has the same significance as in Table II.

The cortex of the left hemisphere is in five cases the thicker, while that of the right is so in four. The maximum difference between the two hemispheres of the same individual is .22 mm. (2.87 to 3.09). The averages for the males and females are nearly alike, the males being a trifle, .02 mm., thicker.

If the results for each locality are averaged for all the controls, these averages arranged in a series from the largest to the smallest and this series plotted as a curve, then the curve has the form indicated by the continuous black ink line on Plate IV. In that curve the insula, as pointed out by Major(64), has the thickest cortex. Next follows the convex surface of the hemispheres with little variation, and then the thickness gradually decreases in the mesal, occipital and orbital cortex, in the order named. Table V gives the figures from which this curve is formed as well as the figures for the two component curves, viz.: that for the males and that for the females.

TABLE V.

Averages for each locality. All controls, I.
Averages for each locality. Controls, Male, II.
Averages for each locality. Controls, Female, III.
Unit of measure, 1 mm.

Locality.	I. Average for all Controls.	II. Average for Controls, (6) Male.	III. Average for Controls, (3) Female.
3	3.38	3.48	3.33
7	3.15	3.02	3.43
6	3.10	3.05	3.18
4	3.09	3.12	3.04
2	3.08	3.06	3.12
5	3.08	3.10	3.04
10	3.04	3.03	3.06
1	2.98	2.92	3.06
13	2.86	2.82	2.94
12	2.75	2.83	2.60
11	2.65	2.65	2.66
8	2.61	2.67	2.50
9	2.53	2.60	2.41
14	2.52	2.59	2.38
	Average,	2.92	2.91

By these figures I aim to show the normal thickness of the cortex at the given localities.

The figures which form the basis for the curve of the Bridgman brain are given in Table VI. The average thickness of this cortex (see Table IV) is 2.59 mm., which is 0.32 mm. below the average for all the females and 0.15 mm. below that for the female in whom the cortex was thinnest.

TABLE VI.

I. Averages of the several localities. L. B., right hemisphere.
II. Averages of the several localities. L. B., left hemisphere.
III. Averages of the several localities. L. B., both hemispheres.

LAURA BRIDGMAN.

Locality.	I. R. H.	II. L. H.	III. Average.
3	3.45	2.98	3.22
7	2.93	2.72	2.83
6	2.26	2.56	2.41
4	2.98	2.77	2.88
2	2.74	2.89	2.82
5	2.61	2.75	2.68
10	2.51	2.41	2.46
1	2.70	2.54	2.62
13	2.81	2.69	2.75
12	2.70	2.56	2.63
11	1.99	2.72	2.36
8	2.16	2.48	2.32
9	1.99	2.27	2.13
14	1.92	2.35	2.14
	Aver. 2.55	Aver. 2.62	Aver. 2.59

The curves for the Bridgman figures are plotted on Plate IV. That for the left hemisphere is indicated by a broken line (dashes), and that for the right hemisphere by the line of long and short dashes. Attending for the moment to these we observe a remarkable drop at 6; from 4 to 12 both curves are generally low with a special depression at 10, and from 12 to the end they run at different levels.

It will be seen at a glance that these two curves are fairly accordant until locality 11 is reached. Here they are widely divergent, approach somewhat at 8, again to diverge at 14.

Taking up the peculiarities of the Bridgman cortex then in the order in which they occur we find the insula (3) thinner on the left side. Both sides very thin at 6, the auditory area. Locality 2, the area for motor speech, is well developed on both sides. From 4 to 13 the development is poor, specially so at 10, area for taste and smell. At 12, the area for dermal sensations, the curve is high again, and from that point on commences the remarkable divergence in the curves of the two hemispheres, that for the left side being much higher at 11, 8 and 14, all of which are within the visual area.

Referring now to the description which I have previously given (op. cit.) of the macroscopic features of this brain, I may briefly attempt to collate them with the measurements of the cortex.

The insula (3) on the left side was found less well developed. It has the thinner cortex. *Vide* Waldschmidt(67).

At the auditory area (6) I could not decide on any macroscopic defect, but have since determmined that the first temporal gyrus at its caudal end, especially on the right side, was abnormally slender. The cortex is decidedly thin on both sides, most markedly so on the right. At the area for motor speech, the left side showed a clear lack of development (depression), but the cortex was not particularly thin for this brain.

At 10, the area for taste and smell, there was a general lack of development, exhibited by the entire temporal lobe. This is easily explained by the slow growth of this portion of the brain, a growth which was quite incomplete at the period when Laura was taken ill (2 years). The glossopharyngeal nerves appeared normal, but the olfactory bulbs and tracts were small, though not so small as in the case of some normal persons. The thinness of the cortex at this point (10) appears therefore as a part of the general arrest in growth.

Passing now to the visual area it was noticed macroscopically that both occipital lobes were blunted, but the right side turned out in every way to be much the more defective and anomalous. Concordantly the cortex of this right side at 11, 8, and 14 is much thinner than that of the left.

It must be recalled here that although at the age of two years, Laura became completely blind in her left eye, yet she retained some remnant of vision with her right eye up to her eighth year. This has left its mark on the entire central apparatus for vision. The right optic nerve is larger than the left.

Area of cross-section of R. optic nerve = 5.00 □ mm.
" " " " " L. " " = 3.38 " "

The relation in the tracts is, of course, reversed:

Area of cross-section of R. optic tract = 3.13 □ mm.
" " " " " L. " " = 4.69 " "

On the one hand then we have loss of vision in left eye at 2 years of age, associated with the smaller optic nerve and tract—a defectively developed right occipital lobe and a thin cortex in the right visual area. On the other hand we have some vision in the right eye up to the eighth year of age, associated with the larger optic nerve and tract, the more normal occipital lobe and the thicker cortex.

The general thinning of the motor cortex I would explain in part by the absence of the fibres through which the motor areas are normally associated with the sensory areas—here defective—and in part by the smaller size of some of the cell elements and non-development of others, resulting from lack of stimuli. The defects in the visual and auditory area follow directly from the loss of the corresponding sense organs and consequent arrest of growth. When the loss is not at first complete a good deal of subsequent development is possible. Why the speech-centre has not a thinner cortex I cannot, at the moment, explain.

In considering the fact that the sensory centers are much more affected than the motor, it should be remembered that aside from the special loss due to arrest and possibly degeneration falling less on the motor than on the sensory centres, there is the physiological difference that each motor centres can be excited by way of any sensory centre, and hence, so long as any senses are left, the motor centres must be stimulated to some degree, while the destruction of the special sense-organ throws a given sensory centre quite out of function. The physiological conditions in the two cases are therefore quite different and in favor of the development of the motor side.

For reference. I introduce here several tables containing the details of the figures just given.

Table VII. gives the maximum and minimum thickness of the cortex as observed at each locality on Laura Bridgman and the nine controls. The maximum was taken at the summit of the gyrus and the minimum at the side — not at the bottom of the sulcus. The average of the maximum and minimum is obtained by doubling the minimum, adding the result to the maximum and dividing the sum by three. This average figure is given in the third column for each hemisphere. The averages at the foot of the first and second columns are obtained by dividing the sum of these columns by fourteen. All the figures in this table are corrected for hardening, so that they represent the natural thickness of the cortex. The observations for the males and females are separated.

TABLE VII.

Females.

Specimen.	BRIDGMAN.						I.						VI.						XI.					
	R. H.			L. H.			R. H.			L. H.			R. H.			L. H.			R. H.			L. H.		
Locality.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.
1	2.91	2.59	2.70	3.49	2.07	2.54	2.91	2.59	2.70	3.43	3.11	3.22	3.17	2.98	3.04	3.75	3 56	3.62	3.36	2.85	3.02	3.17	2.59	2.78
2	3.30	2.46	2.74	3.23	2.72	2.89	3.56	3.24	3.35	3.36	2.72	2.93	3.88	2.91	3.23	3.24	2.91	3.02	3.24	2.79	2.94	3.56	3.11	3.26
3	3.88	3.24	3.45	3.11	2.91	2.98	3.88	2.91	3.27	3.88	3.24	3.45	3.88	3.30	3.49	3.75	3.43	3.54	3.49	2.26	2.67	3.56	2.33	2.74
4	3.24	2.85	2.98	3.40	2.46	2.77	2.98	2.39	2.59	3.69	3.24	3.39	3.43	3.24	3.30	3.24	2 91	3.02	3.11	2.52	2.72	3.36	3.11	3.19
5	3.24	2.30	2.61	3.07	2.59	2.75	3.30	2.81	2.97	3.79	3.33	3.48	3.72	2.94	3 20	3.56	3.07	3.23	2 52	2.20	2.31	3.36	2.91	3.06
6	2.91	1.94	2.26	2.91	2.39	2.56	3.62	3.36	3.45	3.75	3.24	3 41	3.88	3.49	3.62	3.30	2.59	2.83	3.36	2.98	3.11	3.49	2.26	2.67
7	3.24	2.78	2.93	2 98	2.59	2.72	3.56	3.17	3.30	3.88	3.69	3.75	3.56	3.24	3.35	3.56	3.24	3.35	3.56	3.36	3.43	3.43	3.36	3.38
8	2.59	1.94	2.16	3.04	2.20	2.48	2.91	1.94	2.26	2.91	2.01	2.31	3.17	2.98	3.04	3.24	2.39	2.67	2.72	2.26	2.41	2.39	2.26	2.30
9	2.20	1.88	1.99	2.52	2.14	2.27	2.72	1.94	2.20	3.56	2.26	2.69	3.24	2.39	2.67	2.78	2.07	2.31	2.59	1.94	2.16	2.78	2.26	2.43
10	2.84	2.35	2.51	2.75	2.24	2.41	2.85	2.14	2.38	2.59	2.26	2.37	2.88	2.91	3.23	3.75	3.11	3.32	3.88	3.56	3.67	3.88	3.11	3.37
11	2.20	1.88	1.99	2.98	2.59	2.72	3.04	2.91	2.95	3 88	2.39	2.89	3.33	2.56	2.82	3.07	2.59	2.75	2.65	1.94	2.18	2.65	2.20	2.35
12	2.91	2.59	2.70	2.65	2.52	2.56	3.11	2.07	2.42	2.91	2.65	2.74	3.75	2.52	2.93				2.91	2.39	2.56	2.59	2.20	2.33
13	3.24	2.59	2.81	3.04	2.52	2.69	3.43	3.11	3.22	3.43	2.59	2.87	3.24	2.98	3.07	3.36	2.98	3.11	3 30	2.72	2.91	2.78	2.26	2.43
14	2.14	1.81	1.92	2.91	2.07	2.35	2.59	1.94	2 16	2.72	2.39	2.50	3.04	2.59	2.74	2.72	2.33	2.46	2.78	2.07	2.31	2.46	1.94	2.11
Aver.	2.92	2.37	2.55	3.01	2.43	2.62	3.18	2 61	2.80	3.41	2.79	3.00	3.51	2.93	3.12	3.33	2.86	3.02	3.11	2.56	2.74	3 10	2.58	2.74

TABLE VII.—*Males.*

Specimen	II.						III.						IV.					
Locality.	R. H.			L. H.			R. H.			L. H.			R. H.			L. H.		
	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.
1	3.04	2.65	2.78	3.30	2 20	2.57				3.24	2.91	3.02						
2	3.36	2.91	3.06	3.56	3.11	3.26	3.24	3.11	3.15	3.11	2.98	3.02	3.49	2.91	3.10	3.24	3.17	3.19
3	3.56	2.91	3.13	3.88	3.88	3.88	3.88	2.91	3.23	3.56	2.91	3.13	3.24	2.75	2.91	3.88	3.36	3.53
4	3.24	3.17	3.19	3.24	2.98	3.07	3.11	2.72	2.85	3.24	2.59	2.81	3.81	3.56	3.64	3.69	3.24	2.39
5	3.56	2.91	3.13	3.40	2.73	2.95	3.30	2.75	2.93	3.36	2.65	2.89	3.56	3.30	3.39	3.56	3.24	3.35
6	3.56	3.11	3.26	3.56	3.11	3.26	3.56	3.24	3.35	3.24	2.91	3.02	3.88	3.17	3.41	3.56	2.91	3.13
7	3.11	2.91	2.98	3.43	2.39	2.74	2.91	2.59	2.70	3.36	3.24	3.28	3.88	3.24	3.45	3.56	3.43	3.47
8	2.78	2.59	2.65	2.59	2.20	2.33	2.59	2.26	2.37	2 91	2.59	2.70	3.17	2.91	3.00	2.85	2.59	2.68
9	2.91	1.94	2.26	2.65	2.39	2.48	2.59	2.20	2.33	2.72	2.01	2.25	3.24	2.91	3.02	3.24	2.72	2.89
10	3,11	2.91	2.98	3.56	2.78	3.04	3.88	3.24	3.45	3.24	3.24	3.24	3.88	3.56	3.67	3.56	2.91	3.13
11	3.56	3.11	3.26	3.30	2.65	2.87	2.39	1.94	2.09	2.91	2.65	2.74	3.24	2.71	2.89	2.58	2.07	2.24
12	3.36	2.26	2.63	3 36	3.11	3.19	3.24	2.59	2.81	2.85	2.39	2.54	3.24	2.59	2.81	3.49	3.24	3.32
13	3.36	2.59	2.85	3.36	2.59	2.85	2.91	2.26	2.48	2.65	2.52	2.56	3.24	2.85	2.98			
14	2.59	2.39	2.46	2.65	2.46	2.52	2 52	2.07	2.22	2.91	2.72	2.78	3.04	2.39	2.61	2.59	2.39	2.46
Aver.	3.22	2.74	2.90	3.27	2.76	2.93	3 09	2.61	2.77	3.09	2.74	2.86	3.45	2.99	3.14	3.32	2.94	3.07

TABLE VII.—*Males.*

Specimen	IX.						X.						XII.					
Locality.	R. H.			L. H.			R. H.			L. H.			R. H.			L. H.		
	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver	Max.	Min.	Aver.
1	3.56	2.80	3.05	3.24	2.33	2.63	2.98	2.78	2.85	3.49	3.30	3.36	3.36	2.91	3.06			
2	3.69	3.36	3.47	3.36	2.98	3.11	2.59	2.26	2.37	3.11	2.65	2.80	3.24	2.78	2.93	3.56	3.11	3.26
3	4.01	3 88	3.92	3.88	3.49	3.62	3.88	3.56	3.63	3.88	3.56	3.67	3.88	3.56	3.66	3.56	3.36	3.43
4	3.17	2.91	3.00	3.36	2.91	3.06	3.49	3.17	3.28	3.43	3.24	3.30	3.11	2.59	2.76	3.56	2.78	3.04
5	3.23	2 98	3.06	3.49	2.91	3.10	3.24	2.91	3.02	3.88	3.43	3.58	3.56	2.91	3.13	3.24	2.39	2.67
6	3.56	2.33	2.74	3.24	3.17	3.19	3.11	2.72	2.85	2.72	2.59	2.63	3.49	3.11	3.24	3.17	2.26	2.56
7	3.88	2.91	3 23	3.36	2.72	2.93	3.56	2.65	2.95	3.69	2.52	2.91	2 59	2.39	2.46	3.36	2.98	3.11
8	3.24	2.65	2.85	3.56	2.26	2.69	2.98	2.52	2.67	3.30	2.85	3.00	2.78	2.33	2.48	2.91	2.46	2.61
9	2.91	2.59	2.70	2.85	2.46	2.59	2.72	2.59	2.63	2.85	3.34	3.18	2.72	2.39	2.50	2.59	2.20	2.33
10	3.24	2.65	2.85	2.91	2.39	2 56	2.59	2.39	2.46	3.88	3.30	3.49	3.24	2.59	2.81	3.36	2.47	2.73
11	2.46	2.39	2.41	3.11	2.39	2.63	3.36	2.59	2.85	3.43	2.59	2.87	3.11	2.26	2.54	2.59	2.33	2.42
12	3.30	2.59	2.83	3.36	2.52	2.80	2.65	2.46	2.52	3.36	2.78	2.97	3.36	2.26	2.63	3.43	2.59	2.87
13	3.30	2.72	2.91	3.56	3.24	3.35	3.11	2.98	3.02	3.49	2.24	2.66	3.88	2.33	2.85	2.72	2.46	2.55
14	2.91	2.14	2.40	2.59	2.39	2.46	3.11	2 98	3.02	3.04	2.72	2.83	2.59	2.26	2.37	3.56	2.72	3.00
Aver.	3.32	2.78	2.96	3.28	2.73	2.91	3.10	2.75	2.87	3.40	2.94	3.09	3.21	2.62	2 81	3.20	2.62	2.81

Table VIII. is derived from Table VII. by arranging the figures for the average thickness of each locality in each hemisphere in vertical columns, and getting the averages of these for the females alone, for the males alone, and for both together.

TABLE VIII. *Controls Only.*

Locality.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.		
	2.70	3.35	3.27	2.59	2.97	3.45	3.30	2.26	2.20	2.38	2.95	2.42	3.22	2.16	I R.	Females.
	3.22	2.93	3.45	3.39	3.48	3.41	3.75	2.31	2.69	2.37	2.89	2.74	2.87	2.50	I L.	
	3.04	3.23	3.49	3.30	3.20	3.62	3.35	3.04	2.67	3.23	2.82	2.93	3.07	2.74	VI R.	
	3.62	3.02	3.54	3.02	3.23	2.83	3.35	2.67	2.31	3.32	2.75		3.11	2.46	VI L.	
	3.02	2.94	2.67	2.72	2.31	3.11	3.43	2.41	2.16	3.67	2.18	2.56	2.91	2.31	XI R.	
	2.78	3.26	2.74	3.19	3.06	2.67	3.38	2.30	2.43	3.37	2.35	2.33	2.43	2.11	XI L.	
Average: Females.	3.06	3.12	3.33	3.04	3.04	3.18	3.43	2.50	2.41	3.06	2.66	2.60	2.94	2.38		
	2.78	3.06	3.13	3.19	3.13	3.26	2.98	2.65	2.26	2.98	3.26	2.63	2.85	2.46	II R.	Males.
	2.57	3.26	3.88	3.07	2.95	3.26	2.74	2.33	2.48	3.04	2.87	3.19	2.85	2.52	II L.	
		3.15	3.23	2.85	2.93	3.35	2.70	2.37	2.33	3.45	2.09	2.81	2.48	2.22	III R.	
	3.02	3.02	3.13	2.81	2.89	3.02	3.28	2.70	2.25	3.24	2.74	2.54	2.56	2.78	III L.	
		3.10	2.91	3.64	3.39	3.41	3.45	3.00	3.02	3.67	2.89	2.81	2.98	2.61	IV R.	
		3.19	3.53	3.39	3.35	3.13	3.47	2.68	2.89	3.13	2.24	3.32		2.46	IV L.	
	3.05	3.47	3.92	3.00	3.06	2.74	3.23	2.85	2.70	2.85	2.41	2.83	2.91	2.40	IX R.	
	2.63	3.11	3.62	3.06	3.10	3.19	2.93	2.69	2.59	2.56	2.63	2.80	3.35	2.46	IX L.	
	2.85	2.37	3.63	3.28	3.02	2.85	2.95	2.67	2.63	2.46	2.85	2.52	3.02	3.02	X R.	
	3.36	2.80	3.67	3.30	3.58	2.63	2.91	3.00	3.18	3.49	2.87	2.97	2.66	2.83	X L.	
	3.06	2.93	3.66	2.76	3.13	3.24	2.46	2.48	2.50	2.81	2.54	2.63	2.85	2.37	XII R.	
		3.26	3.43	3.04	2.67	2.56	3.11	2.61	2.33	2.73	2.42	2.87	2.55	3.00	XII L.	
Average: Males.	2.92	3.06	3.48	3.12	3.10	3.05	3.02	2.67	2.60	3.03	2.65	2.83	2.82	2.59		
Average: Males and Females.	2.98	3.08	3.38	3.09	3.08	3.10	3.15	2.61	2.53	3.04	2.65	2.75	2.86	2.52		

Table IX. gives the difference in the thickness of the cortex in the two hemispheres of those controls in which the difference is greatest. The figures on which this table is based are found in the "average" columns of Table VII. The controls are grouped into males and females and the instance of greatest difference found for each group. To be compared with this is the difference in the same localities in the Bridgman brain. The figures for the latter show that the differences are much within the extremes of the controls except at those localities where the largest difference is to be expected i. e., 3, 8, 11, 14—where they may exceed those of the controls. The roman numeral indicates the number of the specimen and the side which is larger is first designated, so that VI L.-VI R. means that the left hemisphere has the thicker cortex in control VI. It is not without interest in this case that among the females, 9, and among the males, 11 out of the 14 cases have the left cortex the thicker.

TABLE IX.

Greatest Differences in Cortical Thickness.

	FEMALES.		MALES.		L. B.	
Loc.	Gr. Diff.	Specimen.	Gr. Diff.	Specimen.	Gr. Diff.	
1.	.58	VI L.—VI R.	.51	X L.—X R.	.16	R. L.
2.	.42	I R.—I L.	.43	X L.—X R.	.15	L. R.
3.	.18	I L.—I R.	.75	II L.—II R.	.47	R. L.
4.	.80	I L.—I R.	.28	XII L.—XII R.	.21	R. L.
5.	.75	XI L.—XI R.	.56	X L.—X R.	.14	L. R.
6.	.79	VI R.—VI L.	.68	XII R.—XII L.	.30	L. R.
7.	.45	I L.—I R.	.65	XII L.—XII R.	.21	R. L.
8.	.37	VI R.—VI L.	.33	X L.—X R. III L.—III R.	.32	L. R.
9.	.49	I L.—I R.	.55	X L.—X R.	.28	L. R.
10.	.30	XI R.—XI L.	.54	IV R.—IV L.	.10	R. L.
11.	.17	XI L.—XI R.	.65	III L.—III R. IV R.—IV L.	.73	L. R.
12.	.32	I L.—I R.	.56	II L.—II R.	.14	R. L.
13.	.48	XI R.—XI L.	.44	IX L.—IX R.	.12	R. L.
14.	.34	I L.—I R.	.63	XII L.—XII R.	.43	L. R.

III.—Histological Examination.

The Bridgman brain was not well enough preserved to admit of a very fine microscopical examination. Some points can be made out, however, on sections .02 mm. thick, stained with hæmatoxylin and eosin, or hæmatoxylin and carminic acid, or with Weigert-Pal hæmatoxylin. Whatever general statements are made are always in comparison with the nine controls, from which sections were also cut and similarly stained.

The cells generally in the Bridgman cortex have abundant pigment—the nuclei often somewhat irregular and the nucleoli sometimes single and clear, often multiple and unclear, and, at times, wanting. Where the cortical granules form layers they appear abundant, as a rule, and immature (i. e., without angles), as though they had been arrested in their growth. The general impression one gets is, that the large nerve cells are neither so large nor so numerous as in the normal brains. Of cell processes and abundance of fibres one can only say, that there appear less of both in all localities, and hasten to add, that the poor condition of the material makes itself painfully felt at this point.

It seemed worth while, however, to select sections from several localities, especially those in which the cortex of the Bridgman brain appeared thin, and attempt to get some notion of the development of the cell elements at these points.

To arrive at this result I counted the number of cells above a given diameter in a strip of the cortex, comparing the number found in the Bridgman cortex with that in two controls. For results see Table X.

TABLE X.

To show the average number of cells $12\,\mu$ in transverse diameter which occur in 0.01 □ mm. of cerebral cortex at the localities named. Sections .02 mm. thick.

	MALE.			FEMALE.					
	Control III.			Control XI.			Laura Bridgman.		
Locality.	* R.	† L.	Aver.	R.	L.	Aver.	R.	L.	Aver.
Speech, 2	.85	1.10	0.975	1.06	1.16	1.11	.93	.80	0.865
Insula, 3	1.15	1.04	1.10	1.15	1.03	1.09	1.00	1.07	1.035
Head and Eyes, 4	1.13	1.40	1.26	1.03	1.46	1.25	1.11	1.19	1.15
Hearing, 6	1.23	.99	1.11	1.23	1.21	1.225	.81 c	.92	0.865
Taste and Smell, 10	.82	1.12	0.97	1.34	.97	1.155	.86	1.05	0.955
Sight, 11	1.03	.99	1.01	1.08	.95	1.015	.47 c	1.01	0.74
Sight, 14	1.13	1.08	1.105	.99	1.08	1.03	.40 c	.92	0.66
Average,			1.075			1.125			0.895

* R. = Right hemisphere.
† L. = Left hemisphere.

To obtain these figures the following method was employed. The specimen was fixed upon a mechanical stage in such a way that the direction of motion was vertical to the cortex. It was examined with a Zeiss apochromatic objective, 4 mm. focus, combined with the compensating eye-piece 6, tube 160 mm., thus giving an enlargement of 375 diameters.

The eye-piece carried the micrometer with 50 divisions. With the objective used, each division had a value of 4 μ. The whole scale covered therefore 50 times .004 mm. = .2 mm.

Placing the micrometer scale so that it was at right angles to the direction of motion for the specimen, and passing the specimen in review by means of the mechanical stage, a strip of cortex .2 mm. wide could be brought, throughout its entire extent, under the scale. In this manner the nerve cells were sifted, so to speak, through the micrometer scale, and each one that was 12 μ or more in diameter was picked out and counted.

In selecting the point on the section at which to make this test I always took the spot where the cells were apparently—to a low power—most abundant, and in all cases everything in the field that could be counted was counted.

The depth of the cortex where the count was made was multiplied by the constant width, .2 mm., and the total number of cells divided by this product, using .01 sq. mm. as the unit. The thickness of the section was always .02 mm., which being a constant factor may be neglected. By this treatment it comes out that about one cell, 12 μ or more in basal diameter, normally occurs in each .01 sq. mm. of a section .02 mm. thick.

For comparison with the Bridgman sections I took those from Control III. (Brain weight 1,393 gr., male, average thickness of cortex R. H. 2.77 m., L. H. 2.86 m.), and Control XI. (Brain weight 1,196 gr., female, average thickness of cortex R. H. 2.74 m., L. H. 2.74 m.), (see Table IV.), thus happening to get both the male and female with the thinnest cortex.

Table X. shows that, taking the average of both sides, at no locality in the Bridgman brain are the large nerve cells, as abundant as in the controls. The number in both the controls is nearly the same.

Taking the matter more in detail the motor areas in Laura do not show as great a poverty of large cells as the sensory areas.

In three instances (marked c in Table X.), the abundance of cells accords with the thickness of the cortex—i. e., the thicker cortex has the larger number of cells. These instances include the ones in which the Bridgman cortex most clearly deviates from the normals.

As in the measurements of cortical thickness, so in the abundance of cells, the Bridgman brain is clearly deficient at 6, the auditory area and in the right hemisphere at 11 and 14, visual area, while in the left hemisphere some deficiency is to be noted only at 14, thus again bringing out the contrast between the occipital regions on the two sides. Locality 10 has fewer cells than the controls, but the difference is not so marked as in the thickness of the cortex.

In general it may be added that where the number of cells above 12 μ. in basal diameter was small, that there the absolute number of large cells appeared smaller, and the very largest cells not so large, as in the controls. In other words, small number and small size of large cells appeared to be associated, though I have no figures to present on the point. If, however, my impression is correct, then Table X. only in part represents the difference in the development of the cortical cells of Laura as compared with the controls.

SUMMARY.

I.—General.

1. No figures can be given for the average thickness of the fresh normal cortex. The various investigators differ widely in their results. My own results agree most closely with those of Jensen.
2. Persons with an acquired defect of the central nervous system have a thinner cortex than normal persons.
3. Females have a slightly thinner cortex than males. Difference less than 1%.
4. The right hemisphere (normally) has a cortex a few per cent less thick than the left. Maximum difference 7%.

II.—Special.

1. The cortex of Laura Bridgman was abnormally thin, having but 89% of the thickness of the controls. If we suppose that in its other dimensions the cortex was similarly reduced in development, i. e. by 11% in each linear measurement, then its normal extent would have been 246,808 sq. mm. instead of 200,202.5 sq. mm. as found. This estimate is similar to some of those by the Italian observers, Calori (48) and De Regibus (17-p. 276).

2. The right hemisphere had on the average the thinner cortex—specially to be associated with the defective visual area.

3. The thinning in the motor areas was not so well marked as in the areas for the defective senses.

4. Cortex of motor speech centre was not thin.

5. Cortex of area for dermal sensations was well developed.

6. Auditory areas (6) on both sides and visual area on right side (11, 8, 14) remarkably thin.

7. Area for taste and smell (10) thin—associated with the generally undeveloped state of the temporal lobe.

III.—Histological.

1. The cortex of Laura Bridgman contained an abnormally small number of large nerve cells—i. e., cells 12 μ. or more in transverse basal diameter.

2. There were fewer nerve cells in the samples from the right, than in those from the left hemisphere.

3. The deficiency of nerve cells was not so well marked in the motor as in the sensory areas.

4. In the centre for motor speech (2) the number of nerve cells was abnormally small.

5. Number of nerve cells very small in the auditory areas (6), both sides, and in the visual area (11, 8, 14) on the right side.

6. Some diminution in the number of cells at (10), area for taste and smell. Region generally undeveloped.

7. The small number of cells was associated with small size of the largest cells.

The persistence of vision, though in a very defective form, is still of great importance to the full development of the visual cortex—e. g., right eye and left visual area in Laura.

OBSERVATIONS ON THE OLFACTORY REGION.

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Description of the Specimen.

The specimen submitted for examination was a portion of the ethmoid bone, extending from the anterior base of the crista galli to the sphenoid bone, a small part of the sphenoid being included in it. It contained nearly all the perpendicular plate of the ethmoid. At the sphenoidal end the lateral surfaces were devoid of mucous membrane; towards the frontal end the surfaces were quite covered with the remains of membrane in a ragged condition. The right superior turbinated bone presented a smooth surface marked with grooves. Between it and the perpendicular plate was mucous membrane. Little of the left superior turbinated bone remained, and that which did was rough and without grooves. The entire specimen measured from the extreme frontal to the sphenoidal end, 3 cm.; from the apex of the crista to the farthest point on the perpendicular plate, 2.2 cm.; laterally its greatest measurement was through the horizontal plate of the ethmoid, .5 cm. This line represented the base of two triangles; the apex of one being the tip of the crista, that of the other the farthest point on the perpendicular plate of the ethmoid.

The specimen had been hardened in Müller's fluid, and decalcified in a saturated solution of picric acid, the process being completed in a 1% solution of hydrochloric acid. It was imbedded in celloidin, and most of the sections were stained with Delafield's hæmatoxylin and eosine. Four additional stains were used for nerves, viz.: Upson's carminic acid, Schæfer's nigrosine, hæmatoxylin und carminic acid, and Pal's hæmatoxylin.

Results of the Microscopic Examination.

For the purpose of comparison, I obtained a specimen similar to the one under consideration. This was a portion of the ethmoid bone taken from an elderly man who had been a patient at the Worcester Insane Asylum, and had died there. The presumption would be that this specimen could not be taken as a type of the normal, for it is difficult to suppose that one could pass the greater part of a long life in this climate without having had more or less nasal catarrh. The specimen was, however, healthy in its gross appearance: that is, it was symmetrical, both superior turbinated bones were present; their surfaces were shiny and grooved; the mucous membrane was generally and uniformly distributed between the perpendicular plate and the superior turbinated bones. The next point to consider was its microscopic appearance, and here arose the question, What is our standard for the normal? The work in this region has been done mainly upon the lower animals, and while the results obtained are in the main applicable to the olfactory region of the higher animals, including man, obviously it would be of great assistance to have well-conducted studies upon the olfactory region of man. In an investigation upon the olfactory region of a case of leukæmia Hermann Suchannek(84) has touched upon this topic. He has figured a microscopic section of the olfactory region of a man 40 years old, with a normal sense of smell. The picture agrees with the usual description of this region. It represents a section consisting of a regular row of epithelial cells, resting upon a basement membrane, beneath which are many Bowman's glands, a few blood vessels and nerves, with little intertubular connective tissue. Unfortunately no measurements are given, either of the entire mucous membrane or the epithelium. My specimen presented a different appearance. The epithelial layer preserved for the most part its normal characteristics of a regular row of columnar cells resting upon a row of round cells, the epithelial cells being well formed and distinct. In many places, however, the surface was not so well defined, but was breaking into crowded irregular masses of granular matter, while the subjacent layer of round cells had disappeared, and its place was taken by a mass of round cells, which penetrated deeply the underlying tissue. In these localities the surface layer of cells was thrown into folds which projected above the surface, and also ramified into the mucous membrane, like glands. There was a general increase of connective tissue. The thickness of the entire mucous membrane varied from .16 mm. to .88 mm. Those localities that measured .16 mm., taking as a standard the usual description and the figure of Suchannek, were fairly normal. The epithelium of these regions was particularly healthy. The epithelial layer varied from 30μ to 98μ in thickness (Kölliker quoted by Schwalbe(85) gives 40μ to 98μ as the normal thickness). It was thinnest at the extreme vault of the olfactory fissure.

In the Bridgman sections the thickness of the mucous membrane entire varied from .16 mm. to .64 mm., and the thickness of the epithelial layer from 48μ to 90μ. Taking .16 mm. as the thickness of the normal mucous membrane, I found those areas of the mucous membrane that were of this thickness, far from normal. The surface of the epithelial layer was covered with thin granular matter, and the surface line was very irregular. The cells took the stain poorly, showing that they were degenerating into mucus. In many places the cell bodies had entirely disappeared, leaving a mere outline of their former structures. The row of round cells had disappeared and its place was taken by a mass of cells, now pushing up into the epithelial layer, now invading the membrana limitans. In the sub-epithelial tissue there was a dense deposit of connective tissue. In no part of the specimen was the epithelium healthy. At some points the mucous membrane was entirely devoid of epithelial cells; at others, there was the row of round cells, now single, now two or three deep. In some places these cells were becoming polygonal in shape; again over them was a crowded confused mass of irregular cells breaking away. In some places there were breaks of continuity in the line of epithelial cells, otherwise fairly regular in their size and distribution. There were also places where the surface of the mucous membrane was thrown into elevations. There was generally a large increase of connective tissue, which, in some areas, had replaced everything else. In other areas was abundant infiltration of small, round cells. Bowman's glands were very irregularly distributed and varied much in their character. They presented all gradations from a ring of fairly healthy polygonal cells to a confused mass of granular matter.

The mucous membrane on the right of the septum was much healthier than that on the left. Its thickness was uniform, though in some places there was an increased deposit of connective tissue. The curve into the vault of the olfactory fissure was uninterrupted and regular throughout this side of the specimen. The epithelial cells, though individually undergoing degeneration, were fairly regular in outline. Bowman's glands were numerous in the frontal part of the specimen, but toward the sphenoidal end they had disappeared. Throughout this area were nerves and blood-vessels, with greatly thickened walls. The left side of the specimen presented a very different picture. In the frontal fifth of the olfactory fissure was crowded a mass of connective tissue, in which were nerves, blood-vessels, glands, covered ventrad with degenerated epithelium. Still ventrad to this, the perpendicular plate was devoid of mucous membrane, as was also that part of the superior turbinated bone which remained; the greater part of this bone was either in small fragments or had entirely disappeared. The remaining four-fifths of this side of the specimen was occupied by a fibrous tumor, which was, as it were, in a closed cavity, the mucous membrane of the septum having firmly united with that of the superior turbinated bone, giving in the sections the appearance of a ring lined with epithelium, enclosing the tumor. The tumor sprang from the septum and projected into the superior meatus. Its length from its frontal to its sphenoidal end, estimated by the number of sections in which it was found, was upwards of 1.5 cm. It was irregularly polygonal in shape, and measured at its frontal end 1.12 mm. in height (that is, from the septum to the apex of the tumor) and 1.05 mm. in breadth, while at its sphenoidal end the corresponding measurements were 2.50 mm. and 1.44 mm. Its character changed from the frontal to the sphenoidal end. In the frontal region it was made up of a central column of dense connective tissue, which supported nerves, blood vessels, Bowman's glands, the whole being covered with a layer of epithelium as healthy as that in any part of the specimen. At the sphenoidal end the central column was divided by a fissure, Bowman's glands had disappeared, and the whole tumor was filled with spaces of irregular shape, many of them full of blood corpuscles. Blood vessels remained, but there were few nerves and the greater part of the tumor was devoid of epithelium.

The Nerves.

There were two varieties of nerves in the specimen, a branch of the ophthalmic division of the fifth which passes into the nose through the fissure at the base of the crista galli, and the olfactory nerves. The branch of the fifth, a medullated nerve, was in the main normal. The axis cylinders stood out sharply throughout the greater part of the sections. In some areas, however, they had lost this distinctness and showed signs of beginning degeneration. But the change was no greater than might be expected in a woman of Laura's age.

Before entering upon the description of the olfactory nerves of this specimen, it will be well to discuss briefly the normal and pathological anatomy of the olfactory nerve in general.

The generally accepted view of the non-medullated nerve, of which the olfactory is a type, is that it is made up of the so-called Remak's fibres. Each of these consists of an axis cylinder, a neurilemma, and between the two a nucleated nerve corpuscle from place to place. This fibre has a striated appearance due, according to Max Schultze, to the fibrillæ of the nerve, which are distinguished from the axis cylinders of a medullated nerve in that they individually have no medullary sheath. Boveri(86), on the other hand, has made a careful study of this subject, and concludes that the fibrillæ of Max Schultze are really nerve fibres, each having a medullated sheath. This sheath does not, however, belong exclusively to each nerve. It sustains the same relation to the contiguous nerve fibres that the cell wall of a honey-comb does to the cells. A number of these nerves are surrounded by an envelope of connective tissue, in which are here and there stellate connective tissue corpuscles. There are also within the investing sheath, among the nerves, connective tissue corpuscles, with stellate rays which can be traced very far, even to the enclosing sheath.

The olfactory nerves are subject to certain definite pathological conditions. In the first place, they may be congenitally absent. Injury to the head may cause rupture to the nerves as they pass through the cribriform plate. Excessive stimulation may temporarily or permanently destroy their excitability. Tumors in the brain or cerebral hemorrhage may by pressure cause disease of the olfactory nerves. There may be atrophy of the bulb or nerves, or they may be affected by the degenerative changes of old age. Simple neuritis is a very rare affection (Althaus) (87). Chronic neuritis, due to syphilis, however, is not uncommon. The nerve may become involved in local inflammatory changes in connection with meningitis. Bosworth(88) is of the opinion that a very frequent cause of anosmia from diseases of the olfactory nerves is due to the influence of local inflammatory changes. Thus in acute rhinitis, anosmia persists many days after the inflammatory process undergoes resolution. In severer disease of the nose, where the local inflammatory action persists longer, or is of a severer type, the anosmia lasts much longer, long after the inflammatory action has subsided.

To return to our specimen. The nerves were numerous and were easily distinguished by moderate powers of the microscope (320 diameters.) To get some definite idea of the distribution of the nerves in the different parts of the specimen I selected five slides and counted the nerves on them. One of these sections was from the frontal end, one from the sphenoidal, the other three at regular intervals between them. I also made a count of the nerves of the control specimen under similar conditions, with the following results, (The slide numbered one in each case, was from the frontal end).

Bridgman.		Control.	
1st slide,	1 nerve.	1st slide,	4 nerves.
2nd "	7 "	2nd "	20 "
3rd "	28 "	3rd "	10 "
4th "	32 "	4th "	8 "
5th "	18 "	5th "	15 "
	Total 86		Total 57

This enumeration is of interest in that it shows the distribution of the nerves in the different parts of the specimens, but it gives no reliable information as to the relative number of nerves in the two specimens. It is a difficult matter, even under favorable conditions, to stain the olfactory nerves so as to show the nerve fibres. In neither of these specimens was I able to show the olfactory nerves with the special stains for nerve tissue. The only stain that brought them out at all was the hæmatoxylin and eosine, which did it by virtue of its differentiating the connective tissue. We shall see that this latter was greatly increased in the Bridgman specimen, and it is evident that because of this many more nerves would be detected than in the healthier specimen.

In the Bridgman slides, the nerves were surrounded by a ring of connective tissue which was very thick. Within this ring was a uniformly granular field broken up into smaller areas, and more or less studded with deeply stained dots. With a $\frac{1}{12}$ oil immersion objective, these dots were seen to be stellate connective tissue corpuscles. The areas alluded to above corresponded to the portions of the nerve bounded by the connective tissue envelope in Boveri's sections. Here, as with his sections, the connective tissue corpuscles were upon and within the sheaths. Rather the connective tissue corpuscles of the sheath were where the sheath should be, that place being represented in our sections by a vacant space. As this apparent shrinking was quite general throughout the specimen, I attributed it to the action of reagents. With this power, the nerve presented a regularly mottled appearance, very similar to a section of a frog's olfactory nerve as figured by Boveri, and representing according to his views the cut ends of the nerve fibres. The nerve in its essential elements, therefore, was normal. The connective tissue elements, however, were largely increased.

General Considerations.

It will be interesting now, to gather together the available facts relating to Laura's sense of smell, and the general condition of her nasal mucous membrane during life, and to find, if we can, in the condition of this membrane an explanation of her symptoms.

As an infant she was delicate, being subject to severe convulsions. But later her health improved, and when two years old she is described as being more active and intelligent than ordinary children. At two she had scarlet fever with such severity that for seven weeks she was unable to swallow solid food. Both eyes and ears were affected, suppurating freely (1—p 2). When seven years old she was seen by Dr. R. D. Mussey, Professor of Anatomy and Surgery at Dartmouth College, and in a letter dated April 14, 1837, he thus alludes to her sense of smell: "Her sense of smell is thought by her mother to be less acute than other children, as she very seldom applies any odorous substance to her nose: it is not improbable that this sense may have been impaired by the fever"(1—VIII). In this year, 1837, she entered the Perkins Institution and we find in Dr. Howe's report this note (2-155): "For all purposes of use she is without smell, and takes no notice of the odor of a rose, or the smell of cologne water, when held quite near her, though acrid and pungent odors seem to affect the olfactory nerve." April 6, 1842, Miss Swift, Laura's teacher, made this note(1—p. 107): "Dr. Howe came into the room, while she was having a lesson, peeling an orange. She stopped in the midst of a sentence to say, 'I smell an orange.' We can see a decided improvement in her sense of smell since last year, but she has never noticed any perfume so quickly or at so great a distance before." June 19, 1844, we find this note(1—p. 257): "This is the first season she has ever perceived the smell of a rose or pink, and she now puts all flowers to her nose and is disappointed if they have no perfume. In a letter to Mrs. Howe, dated June 25, 1844, Laura herself says(1—p. 258), "I can smell roses much better than I did two years ago, and it gives me much pleasure in smelling roses."

I find but few observations upon the general condition of her nose. Dec. 14, 1843, Miss Swift made this note(1—p. 215): "She has always been a sufferer from a severe catarrhal affection, and as this shows signs of improvement, we hope for a corresponding one in both smell and taste." In 1878 Dr. G. Stanley Hall, in the course of a series of observations upon her several faculties, examined her nose with this result(6):

"There is no deformity or scarification observable without or from a cursory examination within the nose, and the yellow pigment of the Schneiderian membrane can be seen by a very simple apparatus." Dr. Hall made further this very interesting observation. He described her sleeping with long regular breathing, the teeth slightly apart and the tongue pressed against them and almost between them.

I have received the following letter from Miss Della Bennett, who has been a teacher in the Perkins Institution since 1876:

"Laura Bridgman lived for several years in the same family with myself, and I have conferred with the matron of the cottage, and can answer most of your questions definitely. There was copious discharge from her nose, so much so that she was wont to say, 'My poor nose!' Her handkerchief was in frequent demand, and she used many. Her breath was never offensive. She always breathed through her nose, a habit which she formed when quite young, and her breathing was often accompanied with a gentle whistling sound. I have seen her asleep in the daytime and her mouth was closed, but I cannot tell about the night. She did remove mucus from her throat, and occasionally had a sore throat."

From these notes one gathers that at the age of two, Laura suffered from a severe inflammation of the naso-pharynx, which doubtless extended to her nose: that after her illness she was quite destitute of the sense of smell, entirely so when at the age of eight she entered the Perkins Institution: that at the age of fifteen she could detect with certainty and pleasure moderately pronounced odors: that she had a severe nasal catarrh which lasted her entire life, although it decreased somewhat in severity: furthermore that there was no deformity without or within the nose that could be seen by one not accustomed to examine these parts.

We now come to the consideration of the cause of Laura's anosmia and her partial recovery from it. We have seen that the olfactory nerves were capable of performing their function, and according to Dr. Donaldson (*vide ante*) there was no central lesion that would cause anosmia. We must therefore seek for the cause in the periphery of the nervous apparatus. The two chief peripheral causes of anosmia are obstruction to the inspired air due to deformity of the nose, hypertrophy of the turbinated bodies, nasal polypi or tumors, and atrophic disease. That there was not atrophic disease is shown by the absence of bad odor, by the partial return of the sense of smell, and by the result of our examination of the specimen. Furthermore, according to Bosworth, catarrhal affections caused by febrile diseases and prominently scarlet fever, are characterized by hypertrophic changes (88—p. 157). It is quite improbable that Laura had any deformity of the nose or hypertrophic disease in the respiratory part of the nose, which would interfere very materially with the access of the inspired air to the olfactory region, and it is in this latter region, therefore, that we must look for the cause of her anosmia. We have found in the left superior meatus an adequate cause for a complete absence of the sense of smell for that area, in the extensive disease there which resulted in a thorough disorganization of the mucous membrane in a part of the olfactory fissure, while the rest was excluded from all contact with the inspired air by the firm union of the mucous membrane of the septum with that of the left superior turbinated body. In the right superior meatus, on the other hand, conditions were more favorable for the proper performance of function. It is here that Laura must have smelled, and the questions now to be settled are, how could this area have been rendered incapable of performing its function, and how could this function have been resumed.

Catarrhal inflammation of the nasal mucous membrane is the usual accompaniment of scarlet fever, except in the mildest cases, and is associated with an irritating discharge from the nose (Smith) (89). The inflammatory process in these cases does not involve more than the epithelial layers. But in severe disease the deeper tissues of the mucous membrane are affected. There is a copious proliferation of cells in the deeper layers, with fibrinous infiltration even to the extent of compressing the vessels and making portions of the tissue gangrenous (Henock) (90). There may even result necrosis of the bones (Thomas) (91). There may be recovery even though the disease be severe, or it may result in chronic disease with more or less profuse discharge and extensive inflammatory infiltration, or there may be an osteitis of all the bones which enter into the composition of the nasal cavities (Allen)(92).

That Laura's nasal mucous membrane was profoundly affected by the fever there seems no doubt, and it is easy to conceive how the active cell proliferation and swelling of the mucous membrane caused by the catarrhal process would have so affected the delicate termination of the olfactory nerves that they would be entirely incapable of functioning. But as time went on we know her catarrh grew better and we rightfully infer that the inflammatory processes in the mucous membrane subsided, to an extent, though they never entirely ceased. We have seen that the structures of the nose were a good deal damaged, yet they were not entirely useless. In the right superior meatus especially, there were spots of membrane in a fairly healthy condition. A question of interest here presents itself—would the olfactory nerves after so long a period of inactivity preserve their power of responding to stimuli? The following case reported by Allen (92) proves that this is possible. The patient was a married woman. She had never breathed through her nose and had never experienced the perception of an odor. There was found to be a complete bony occlusion of the posterior nares. This was broken through and on the sixth day after the operation she began to smell and in a short time became familiar with the common odors and flavors. The odoriferous air was not kept from Laura's olfactory nerves by bony obstruction, but it was kept from them by what acted as efficiently for a long time, namely, masses of rapidly proliferating cells, and the mucus and débris of a diseased mucous membrane. When this process subsided it again became possible, in those areas where the ephithelium still remained sufficiently healthy, as it did in places, for the terminal filaments of the nerves to receive and convey their proper stimuli. There may have been a further cause for the anosmia. When discussing the pathology of the olfactory nerve, we alluded to Bosworth's view that anosmia was due in some cases to the local action of the surrounding inflammation upon the nerve itself. As I understand the matter he bases this view solely upon clinical experience, and attempts no explanation of the tardy return of the sense of smell after the subsidence of the inflammation. We have in our sections a possible explanation of this peculiarity. The connective tissue of the nerve was increased in amount, while the nerve tissue proper was apparently normal. Interesting questions suggest themselves in this connection. Does the development of this tissue impair the functioning power of the nerve, and does a nerve so affected resume its normal activity more slowly than the surrounding tissue? At present, so far as I know, there is not sufficient anatomical data upon which one could even discuss these topics.

Summary.

I. The ethmoid bone and the mucous membrane covering it had suffered from inflammatory disease, which particularly affected the left side. 2. This disease resulted in an excessive production of connective tissue, and in one area, the left superior meatus, there had been formed a fibrous tumor. The epithelium was generally and considerably diseased. The nerves contained an excess of connective tissue, but were otherwise normal. 3. When two years old, Laura had scarlet fever, which left her anosmic and with severe nasal catarrh. She partially recovered from both these conditions. 4. The anosmia was due to the occlusion of the left olfactory area by the union of the mucous membrane of the septum with that of the superior turbinated body, and also to the action of the inflamed mucous membrane upon the nerves of the right olfactory region. Partial recovery resulted from subsidence of this inflammation.

II.—The Visual Apparatus.

When Laura recovered from her illness it appeared that she was totally blind in her left eye but could see somewhat with the right. The remnant of vision in her right eye continued up to the eigthth year of her life.

From that time on she was absolutely blind in both eyes.

In 1878 Dr. O. F. Wadsworth, of Boston, tested her for vision and found her totally blind ([6]) and at the same time reported on the appearance of the eyes as follows:

"On both sides the lids are sunken, partly on account of lack of the normal amount of orbital fatty tissue, partly on account of the small size of the eyeballs. They remain constantly closed. The right conjunctival sac is much smaller than normal, somewhat irregular, and presents an appearance such as is seen after severe and long-continued inflammation. The right eye appears about one half the normal size. It is wholly enclosed by the sclerotic, except over a space at the centre, some two millimetres in diameter, where a less opaque tissue, on which a few blood-vessels are visible, represents the altered remnant of the cornea. The left conjuctival sac is somewhat larger than the right, and more regular, though still small. The left globe also is a little larger than the right, and its opaque altered cornea is some four millimetres in horizontal and two millimetres in vertical diameter. There was constant irregular oscillation of the globes [nystagmus] whenever they were exposed to view by raising the lids, and the oscillation evidently continued even after the lids were closed."

At the autopsy the eyes were removed with the surrounding tissue and put unopened into the Müller's fluid and alcohol. The hardening was completed in alcohol.

Both bulbs were enclosed by orbital fat. All the muscles the of bulbs were present, though small, and the external appearance of the bulbs corresponded with Dr. Wadsworth's description given in 1878. After hardening, the right eye had a transverse diameter of 15 mm. and an antero-posterior diameter of 10.5 mm. Similar measurements of the left eye gave 17.5 and 11. mm. showing the left to be decidedly the larger. The condition of phthisis bulbi existed for both eyes. There was a faint indication of the anterior chamber. The locality of lens and vitreous contained abundant calcareous deposits in small masses and the choroidal pigment was very abundant. Sections through the point of entrance of the optic nerve showed no trace of the retina or normal nervous elements at this point. Both eyes were similar in the appearance just mentioned. As has been stated the optic nerves were small :

Right optic nerve, area of cross-section near chiasma, 5.00 sq. mm.
Left " " " " " " 3.38 " "

The connective tissue was vastly increased in both nerves but one also saw the characteristic cross sections of axis-cylinders with their medullary sheaths. The fibres were both large and small. It is worth noting that these fibres were abundant in the left nerve but much less so in the right, although the right was the larger nerve. The chiasma was much flattened dorso-ventrally. The optic tracts were small and flattened. Their area was taken about 10. mm. behind the chiasma. The relations of size were of course reversed at this point and the left tract was the larger:

Right optic tract near chiasma,	3.13 sq. mm.
Left " " " "	4.69 " "

From these measurements the only conclusion that can be drawn is that a large part of the fibres decussated. In the tracts, which were not very well hardened, the fibres visible in cross-section of the corresponding optic nerves were also to be found. Throughout the nerves and tracts, but more abundant in the latter, there were numerous droplets or spherical homogeneous masses, as a rule about 12 μ in diameter, and staining with fuchsin and carmine. Lying at the periphery of both nerves and tracts these bodies would appear to correspond with corpora amylacea, with some of the descriptions of which, however, they do not exactly agree. Further than the tracts it was not practicable to carry the histological examination of the optic pathway.

The corpora geniculata externa were too imperfect for description. The pulvinar and the anterior pair of the corpora quadrigemina were both slightly less prominent than in the normal brains. The cortex was the next locality studied and the results there obtained have already been given.

The first point calling for remark is that the eye in which vision was longest retained ultimately had the smaller bulb and at the same time it was associated with the larger optic nerve and tract. The nerve and tract, however, though larger showed fewer nerve fibres that were clearly marked. It should perhaps be noticed in this connection that this smaller bulb had also the smaller (right) oculo-motor nerve in connection with it.

From these facts it would appear that although in general the right eye was more seriously affected yet some portion of the retina remained undamaged for a long time—up to the eighth year. During this period the optic nerve, the tract and the cortex underwent considerable development so that the subsequent degeneration of the right nerve was accompanied by far less atrophy than that of the left side. On the left side the disturbance in the eyeball was in general less severe and though vision was abolished very early, there was left some condition which favored the better preservation of those nerve fibres which did not at an early period undergo degeneration and absorption. I had expected to find complete degeneration of both optic nerves such as had been described by Purtscher. (80)

On the bases of these specimens, I should hardly like to enter into the forms of degeneration possible to the optic nerves but if a double set of fibres in the optic—the two sets developing and conducting in opposite directions—be accepted, then these nerves found intact in this case might be considered as belonging to that set the centre for which was central and which conducted peripherally. v. Monakow (81, 82)

In this instance then the disturbance in the cortex is probably to be looked upon much more as due to an arrest of growth following the removal of the normal stimuli, than to a continuation of the degeneration into the hemispheres.

III.—The Auditory Apparatus.

From the time of her illness to her death there is good evidence that Laura was entirely deaf. At the same time she had a good sense of direction and of equilibrium and was sensitive to rotation. Hall (6). The equilibrium and auditory functions of the eighth nerve are therefore to be separated in this case.

An examination of the ears was made in 1878 by Dr. Clarence J. Blake who reported as follows: (6)

"Both external ears normal. The right external auditory canal normal in size and contour, and the skin lining the passage healthy and showing no marks of previous inflammation-processes. The right membrana tympani was entirely destroyed with the exception of a narrow rim, the remains of the inferior and posterior portions of the membrane, from which a thin cicatrical tissue extended inward to the promontorium over the stapes and fenestra rotunda. The malleus and incus had disappeared. The mucous membrane of the tympanic cavity presented a normal appearance, with the exception of one spot on the promontorium covered with a thin crust of dried secretion about two millimetres in diameter. A band of thin cicatrical tissue also extended across the anterior portion of the tympanic cavity. The left external auditory canal was filled with dark brownish cerumen, on removal of which the passage was found to terminate, at a depth of two centimetres, in a diaphragm of secondary granulation-tissue, concave, very firm, and resisting gentle pressure with a probe, except at the central or thinner portions, where it could be slightly depressed. Its outer covering was continuous with the dermoid lining of the canal."

After death, the petrous bones were put in Dr. Blake's hands and the report on them, made by Dr. W. S. Bryant, of Boston, is the following:

The Examination of Laura Bridgman's Petrous Bones.

The Right Petrous Bone.

A deep groove for the superior petrosal sinus is seen. The external auditory canal is terminated by a concave curtain of fibrous tissue resting on the promontory. There is no evidence left of the former position of membrana tympani except at the floor of the canal, where there is a slight indication of the sulcus tympanicus. The tympanic cavity is considerably constricted by hyperostoses. The oval and round windows are ossified across and the promontory is very rough, leaving only a small space inferiorly and posteriorly. The inferior anterior wall of the tympanum is very thin and there are two pin-hole perforations into the carotid canal.

The Eustachian tube is impervious; its tympanic end being closed by bone and just beyond this there is an accumulation of cheesy matter also enclosed by bone. There are no air spaces within the tympanum for all the bone cells are filled with tissue, although in the highest part of the petrous bone there is a cell which connects with the tympanum. There is no evidence of mastoid cells or antrum. (I did not see the mastoid process).

The chorda tympani muscle is very much atrophied and its tendon is attached to cicatricial tissue. The stapedius was very much atrophied and its canal narrowed. The tendon still protrudes from the tubercle.

Anteriorly and externally the osseous wall of the aqueduct of Fallopius is wanting. No trace of the ossicles could be found. The inner ear appears normal.

Dr. H. F. Sears kindly examined the terminations of the auditory nerve and organ of Corti and found the terminal ganglion cells intact.

The Left Petrous Bone.

The groove for the superior petrosal sinus is unusually deep. A diaphragm of dense fibrous tissue especially thick and firm on the surface and concave outwards forms the end of the conical external auditory meatus 8 mm. external to the base of the styloid process.

The floor of the osseous meatus is defective externally and is pierced internally and anteriorly by a foramen 1 mm. in diameter, in the fissure of Glacier.

External to the fibrous diaphragm there is a diaphragm formed by hyperostosis of the walls of the canal which obstructs the passage except near the centre and slightly external to the normal position of the membrana tympani, where there is an opening 2 x 4 mm.

The hyperostosis extends into the tympanum filling the greater part of it, but leaving a space external to the fenestræ and below the promontory, also a considerable space in the external anterior and superior part of the petrous bone.

There are no air spaces between the place of closure of the meatus and the pharyngeal end of the osseous Eustachian tube. All the bone cells are filled with soft tissue and the osseous Eustachian tube is not seen. No remains of the membrana tympani could be found.

Before I saw the specimen the tympanum had been opened and some of its contents taken out; all of this was lost except the head and neck of the malleus with the base of the long process, all enclosed in fibrous tissue.

The relations of the fenestra ovalis and the attachment of the tensor tympani muscle had also been destroyed. The chorda tympani nerve was found intact. The tendon of the stapedius muscle was protruding from its tubercle.

The aqueduct of Fallopius and its contents are intact. The round window is closed by dense fibrous tissue. Both the round and oval windows are small, less than one-half of normal size.

Dr. H. F. Sears kindly examined the nerves and muscles and found the tensor tympani considerably and the stapedius slightly atrophied. He also found numerous ganglion cells in the cochlea.

The original report of Dr. Bryant ends here. In answer to a further question, however, he states that nothing pathological could be definitely made out in either the cochleas or semi-circular canals. As the original preservation of the specimens had been in Müller's fluid only, they were not in the best condition for a fine histological examination.

As the case stands the inflammation of the middle ear is the occasion of the deafness. The authorities on the subject state that absolute deafness does not follow disease of the middle ear alone. So that there is something here to be explained. I consider that the cochlea must have been thrown out of function on both sides since the tuning fork placed on the skull gave no auditory sensations—and this, to my mind, outbalances the negative result of the histological examination.

The auditory nerves were studied only by means of the stumps attached to the medulla; the right auditory had an area of 4.26 sq. mm. in cross section. The left of 3.17 sq. mm. Both samples were taken within about 3 mm. of their attachment to the medulla. (For the method see the article in this same number on "The size of several cranial nerves in man as indicated by their cross-section.") Roughly their area was about two thirds of that of the similar nerves from the brain of a normal male in whom the cranial nerves were all very large. There is no reason then to think that in Laura the nerves were remarkably small. The figure for the area of the larger, right nerve, is somewhat too high owing to the obliquity of section and some distortion, so that they were really more nearly equal than these figures would indicate.

The connective tissue in the nerve trunks is normal. The nerve fibre show well marked sheaths and axis cylinders. If degeneration has occurred in these nerves the indications of it have long since disappeared. The nerve fibres found would be designated as normal. The bundles of larger fibres, presumptively connected with the semi-circular canals, contain particularly well preserved fibres.

In the medulla both roots and all three nuclei can be clearly identified on both sides.

The fibres in the medulla stain by Weigert's method and the cells with carmine, as well as could be expected from the condition of the specimen. If there is any abnormality it is that the auditory fibres do not take the Weigert's stain particularly well and that the cells of the accessory nucleus in the medulla are few and poorly developed. The striae acusticae were well developed and on gross examination—when the floor of the fourth ventricle was viewed from above—there were visible two bundles on the right side and three on the left which could be counted as belonging to the striae, while just cephalad to these was a well marked bundle on each side of the middle line, corresponding with the structure described as the *conductor sonorus* (Klangstab) and supposed to form part of the centripetal pathway for the auditory impulses.

On comparison with a number of normal specimens the caudal pair of the quadrigemina exhibited no marked peculiarity. They were small, but no smaller than in the case of some normals. The corpora geniculata interna did not appear small in Laura upon gross examination but this appearance I am inclined to attribute to the failure of the surrounding regions to fully develop, thus causing the corp. gen. int. to stand out with unusual clearness.

The next point examined in the auditory pathway was the cerebral cortex and the results there found have already been stated.

I wish to add in this place that in the description of the surface of the brain previously given Iwas not willing to admit any superficial abnormality in the region of the first temporal gyrus at its caudal end. Since writing that description I have made further comparisons with normal brains and have obtained evidence of lack of development in the cortex of this region in the case of Laura. At present then I look on the slenderness of this gyrus, especially on the right side, where the cortex is most affected, as an expression of the incomplete development of the region. Mills (42, 68), Starr (41), Manouvrier (83).

At first sight the small disturbance—to the naked eye at least—existing between the middle ear and the cortex is striking. Histological investigation up to the centres in the medulla yields a similar negative result. Between the medulla and cortex the condition of the specimen did not warrant a histological study.

In the scattered literature relating to the examination of the ear and brain in deaf-mutes, a condition where there is little or no apparent abnormality of the inner ear, the auditory nerve or the medulla, associated with disease of the middle ear, deafness and (sometimes) atrophy of the cortical auditory centres, is occasionally described: Bremer (69), Larsen & Mygind (70), Moos (71), Mygind (72), Obersteiner (73), Moos and Steinbrügge (74, 75, 76). I believe that in future cases, like that of Laura, a more detailed examination than it was possible to make in her case will show disease of the membraneous cochlea or the nerves between it and the spiral ganglion of the cochlea. Such a case has been reported by Moos and Steinbrügge (79).

As long, of course, as the cells of the spiral ganglion are intact, just so long will the auditory fibres associated with them—and this must represent a very large portion of the cochlear division of the auditory—remain morphologically intact. Following the pathway to the cortex we find no point at which marked changes occur until we reach the cortex itself. The disturbance here is most probably due to the early and long continued lack of normal excitation, for the cortical cells in the sensory areas are peculiarly dependent for their proper development on the special sense with which they are associated.

The evidence from stimulation of the cortex and from the histology of the medulla goes to show that the association between the auditory nerve and the cortical centre for hearing is to some extent at least, a crossed one. If this were so, then the smaller, left nerve, would associate itself with the thinner, right cortex. This relation exists in the case of Laura, but it remains for further investigation to show its significance. Strümpell (78).

As regards the semicircular canals it may be added that they were not found diseased. Their nerve was in good condition, and sensibility to rotation, sense of direction, etc., were present. Of course the relation of this part of the inner ear to the middle ear is less intimate than that of the cochlea, and this in part may account for the normal preservation of the canals. That both portions of the labyrinth need not be conjointly affected is shown by James (79), in his study of the sense of dizziness in deaf-mutes, where this sense was found totally lacking in only 186 out of the 519 cases examined.

IV.—The Cranial Nerves.

It is desirable to bring together the various facts regarding the cranial nerves in Laura's case. After what has been said in the foregoing pages, and the discussion of their area by Mr. Bolton and myself (vide p. 224), this can be briefly done. Table XI. gives the various points in a condensed form.

TABLE XI.

NERVE.		AREA IN SQ. MM.	CONDITION.	SIZE.
I. Olfactory, bulb,	right	6.34	Somewhat atrophiad	Small
" " tract,	right	1.46	" "	"
II. Optic nerve,	right	5.00	Greatly atrophied	Very small
" " "	left	3.38	" "	" "
" " tract,	right	3.13	" "	" "
" " "	left	4.69	" "	" "
III. Oculomotor,	right	3.17	Normal	Large
" "	left	3.51	"	"
VIII. Auditory,	right	4.26	Somewhat atrophied	Small
" "	left	3.17	" "	"

The sixth nerve—abducens—contained only normal fibres and appeared healthy, but the measurements on the two sides were so different that I suspect some strands were lost, and hence do not give the figures for the area.

The only nerve in the Table which has not been discussed is the olfactory. The bulb was flattened and the glomeruli could not be identified. The ganglion cell layer was there, and contained some well formed cells. The other layers were poorly preserved. The vessel walls were thickened. There was some excess of connective tissue and an abundance of hyaline bodies—corpora amylacea (?). Distinctly degenerated fibres could not be made out in the tract, but the vessels, connective tissue, corpora amylacea, were found as in the bulb. Grossly the left tract and bulbs were like the right, but by accident the former was lost before it had been examined histologically.

Whether there was anything peculiar in the glossopharyngeal fibres I am unable to say. The portion within the medulla was normal.

The medulla which was examined from the level of the pyramid to the middle of the pons, by means of sections, showed no abnormality save in the neighborhood of the accessory nucleus of the auditory nerve, where the cells appeared small, reduced in numbers and highly pigmented.

The pia of the hemispheres had a normal abundance of nuclei in it, even over the occipital region—and the blood vessels were normal in size and thickness of their walls. The cerebellum was also normal.

V.—Conclusion.

From these fragmentary observations, which leave so many points connected with this special case still undecided, it will be advantageous to construct some sort of general picture.

The anatomical condition was that of a normal brain in which the olfactory bulbs and nerves, the optic nerves, the auditory nerves, and possibly the glossopharyngeal, had all been more or less destroyed at their peripheral ends. This destruction caused a degeneration—most marked in the optic nerves—which extended towards the centres and involved them indirectly. This condition has left its mark more or less plainly on the whole brain, as indicated by the extent and thickness of the cerebral cortex, and specially by the cortex connected with these deficient sensory nerves. The physiological effect of the peripheral lesions, as I conceive it, was to retard growth in the centres, cortical and subcortical, which were thus involved, and also to interfere with, if not entirely prevent, the formation of the association tracts.

To be sure this case represents a maximum loss in these defective senses with a minimum amount of central disturbance, thus offering the very best sort of opportunity for education by way of the surviving senses. At the same time, we must imagine the hemispheres to have been traversed in every direction by partly or completely closed pathways. The brain was simpler than that of a normal person, and Laura was shut off from those cross-references between her several senses, which usually so facilitate the acquisition of information and the process of thought. Mental association was for her limited to various phases of the dermal sensations and the minor and imperfect senses of taste and smell. Yet from their fundamental and protean character, the dermal senses are perhaps the only ones on which alone the intellect could have lived. We are thus brought back to Sanford's (7) conclusion as derived from the study of her writings. "She was eccentric, not defective. She lacked certain data of thought, but not, in a very marked way, the power to use what data she had."

One word more upon the cortex. The deficiency in the motor speech centre is mainly macroscopic, as far as the third frontal gyrus is concerned. The motor centre there had lost some, but not all its associative connections. Histologically, it was slightly deficient. The lesion there was so different from that of the sensory centres that a histological difference ought not, perhaps, to be surprising. The cortex of the sensory centres was not sunken below the surrounding level, though the gyri were slender and flattened. Possibly in this sinking in a motor area and the absence of the same in the sensory areas, we have a suggestive difference in the reactions of the several portions of the cortex.

Finally, the deficiency was not so very great even in those areas, where it was most marked, and the question arises as to what sort of occupation the cells in those areas had, which would thus justify their prolonged existence. If they were thrown entirely out of function it is not easy to see how they could last so well for nearly sixty years. In some way then they may have taken a slight part in the cerebral activity, but it was so slight that their specific reactions did not rise into consciousness, for though Laura had some light perception up to her eighth year, she apparently had no visual memories, whereas those who have retained full vision up to four and a half or five years of age and then become blind, do usually remember in terms of sight (8).

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CORRIGENDA. I. ARTICLE.

Page 304. The percentage increase in volume is certainly too large. It should be one or two per cent. less than that for weight.

Page 306. Line 8. All the specimens mentioned in this paragraph except the Bridgman, are supposed to have been weighed with the pia on. To make this specimen comparable then its weight must be increased by the weight of the pia, 31.4 grms. This makes the total weight of the Bridgman encephalon, with pia, 1235.4 grms.

Page 312. Line 13. Topinard's Table (Éléments d'Anthropologie générale, Paris, 1885) in his Anthropology, p. 518 shows the relations between brain weight and age. It is based on 1913 cases of Boyd, and according to it the maximum encephalic weight for females, falls between the ages of 20-30 years; that for males between 30-40 years; This indicates brain growth up to the age of maximum weight, therefore beyond the twenty-fifth year.

Page 324. Table. The first series of weights stands under the heading "Weight of cerebral hemispheres, fresh." The question arises whether "cerebral hemisphere" should not be replaced by "encephala." I have not seen any account of how much of the encephalon was used in determining the fresh weights in this series, but, since these brains were directly compared with those of other observers in which the entire encephalon had been weighed, it is only fair to suppose that they had been treated in the same way. This was my opinion until I found a table in R. Wagner's(21) Vorstudien, 2te Abhandlung, 1862, P. 91, in which the weights of the two "hemispheres," of at least three of these brains in the table, are compared with one another. The specimens had been in alcohol the strength of which is not given. Now the sum of the weights of the two "hemispheres" is nearly equal to or more than the weight of the "brains" given by H. Wagner (44) 1864. I therefore used the word "hemisphere" in the above heading as equivalent to hemicerebrum. It would appear that both the Wagners used it as equal to hemiencephalon. In the above mentioned table then the weights given are these for the entire encephalon and not for the cerebrum only.

Page 328.	Table I.	*For* absolute difference,	1398.4	sq.	mm.
		Read " "	1598.4	"	"
		For in percentage,	1.8%		
		Read "	1.9%		
Page 334.	Table VIII.	*For* total (left),	101256.0	sq.	mm.
		Read " "	101255.2	"	"
		For absolute difference,	2309.5	"	"
		Read " "	2308.7	"	"

PLATE III.

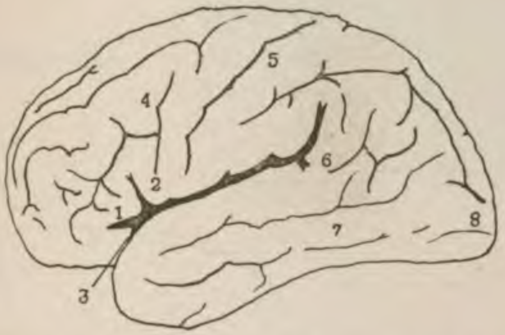


FIG. 1. Lateral aspect. 3 is used to designate the insula here not exposed.

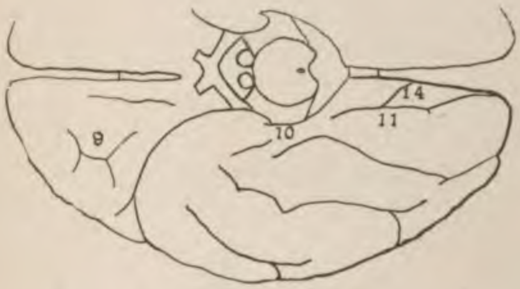


FIG. 2. Ventral aspect.

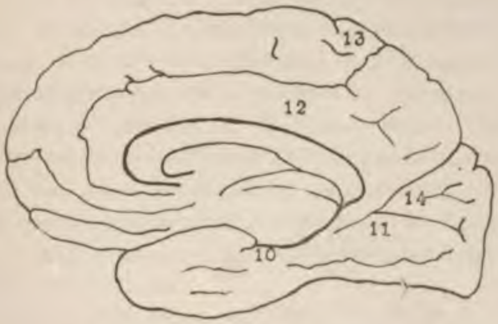
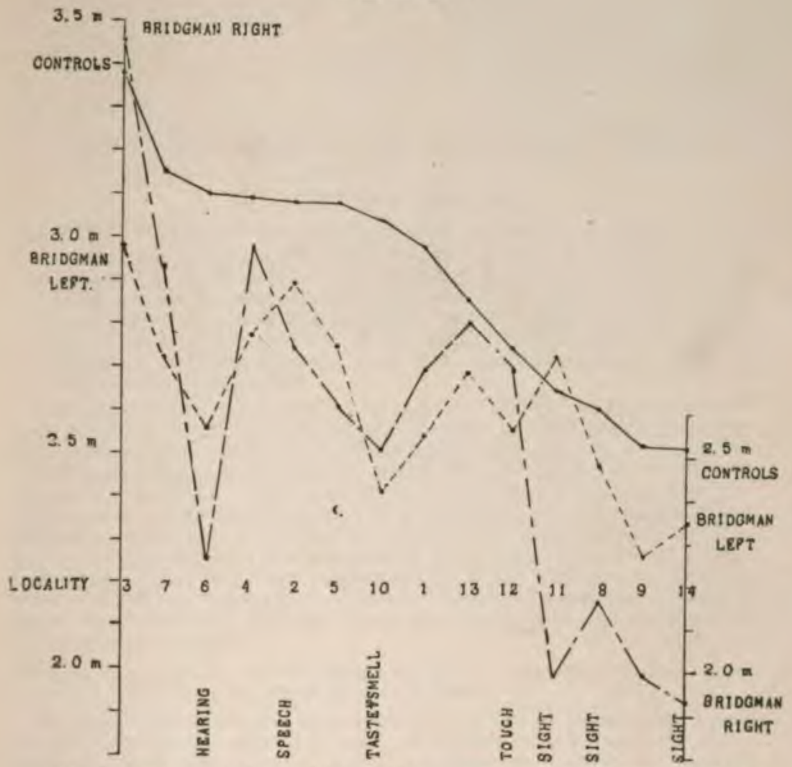


FIG. 3. Mesal aspect.

Explanation of Plate III. This plate shows the localities on the hemispheres from which the samples of cortex were taken. For the physiological value of these localities Table III may be consulted.

PLATE IV.



Explanation of Plate IV. The curve was originally plotted so that the thickness of the cortex was magnified 100 times, i. e., .01 mm. of cortex corresponded to 1. mm. on the ordinates. The original has been reduced for printing to somewhat less than six-tenths of its linear scale. The figures placed by the ordinates indicate the thickness of cortex. The summits of the curves are alone represented, there being 1.8 mm. of cortex below what is shown. The figures for the localities cross the plate in a horizontal line, with the important designations below them.

The curve for all controls is in a solid line, ———
The curve for L. B., right hemisphere, is in long and short dashes, — - —
The curve for L. B., left hemisphere, is in short dashes, - - - -

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

Ueber das Verhältniss der experimentellen Atrophie und Degenerations-Methode zur Anatomie und Histologie des Centralnerven systems. Ursprung IX, X, und XII Hirnnerven. Dr. AUG. FOREL unter Mitwirkung von Dr. MAYSER und Dr. GANSER. Mit einer Tafel. SEPARAT-ABDRUCK AUS D. FESTSCHRIFT DES FÜNFZIGJÄHRIGEN. Doctor JUBILÄUM der Herrn Prof. Dr. KARL WILHELM von NÄGELI in München und Geheimrath Prof. Dr. ALBERT von KÖLLIKER in Würzburg. Zürich, 1889.

This paper explicitly contains nothing new. In his clear and emphatic way Forel sets forth the value of the method of experimental atrophy and degeneration, and shows the utter impotence of the view that a so-called anatomical problem is to be dealt with by means of traditional anatomical methods. Any method, or better, every method which is applicable must be employed, and only results which are obtainable by several methods have a right to be regarded as well established. The method of degeneration is illustrated by what it has contributed to our knowledge of the medullary centres of IX, X and XII nerves. To those who wish to know what the method of v. Gudden is and can do, and to those who are aweary with the much reading of sapless anatomy, this paper will be a delight.

Cerebral Localization. DAVID FERRIER, M. D. Croonian lectures—Lancet. June 7, 14, 21, 28, July 5, 12. 1890.

In these six lectures the author goes over the entire subject, laying special stress on the centres in man. For these lectures some new experiments have been specially made, and these are of particular interest from the bearing they have on Ferrier's own views.

The first lecture opens with an account of the comparative physiology of the cerebral hemispheres, in which the author draws largely on the work of Steiner, Schrader, Goltz and others. The reactions of the shark, bony-fish, frog, bird (pigeon), and mammals (rabbit and dog), are described, after more or less complete removal of the cerebral hemispheres. Following this is a brief historical account of the work on localization, in which Ferrier points out the unsatisfactory nature of the evidence for absolute and relative centres, as advocated by Exner, gives Beevor's figure for the relations of the fibre-bundles in the internal capsule—as derived from recent experiments on direct stimulation in that locality—and passes to the arguments in favor of the direct excitability of the cortical cells—the best of which are the tetanic response to single stimuli, and the longer time taken for reaction when the cortical cells are present.

The second lecture deals with the results of electrical stimulation of the cortex—mainly in monkeys—and diagrams of the localizations of Beevor and Horsley and others are given. Schäfer has reported movements of the eyes in monkeys which stand in a d[illegible]e relation to the portion of the occipital lobes stimulated. [illegible] general fact, but still contends that more preci[illegible] from the stimulation of the angular gyrus [illegible] he results of stimulation of the cortex in man, the author alludes to the completeness with which centres may be separated. His position has been that of a complete separation, and although he does not abandon this view now, he nevertheless argues for the great difficulty of demonstrating it. In considering the visual centre, Ferrier sees in the experiments of Schäfer just mentioned and those on the latent period of excitation for this region a confirmation of his view that the motor reactions here obtained are reflexes from sensory stimulation. The evidence is by no means convincing. The visual area is the occipito-angular region. The relative values of the angular and occipital regions are by no means easy to determine, but Ferrier grants more significance to the occipital region than on previous occasions. It appears that injury to the occipital lobes causes crossed hemiopia, while injury to the angular gyrus produces blindness or amblyopia of the opposite eye. Destruction of both the angular gyrus and the occipital lobes is the only lesion which gives a permanent result. Brown and Schäfer's further investigations would appear to show that removal of the occipital lobes alone is capable of producing a complete and permanent blindness. Ferrier would explain this result by incidental injury to the angular gyri. For Ferrier the angular gyrus is the centre for central vision. As against Munk, he points out that no sensory disturbance—save the visual one—follows its removal, and that the gyrus has some slight connection with the eye of the same side. On reviewing the clinical evidence bearing on the visual centre, Ferrier does not find it to support the views of Seguin and Nothnagel—that the cuneus is the most important region in man, but would explain the connection between lesions of the cuneus and disturbances of vision by injury to the optic radiation thus brought about. In the discussions of the visual centres in the lower vertebrates Ferrier introduces an interesting experiment in which the physiological proof for partial decussation of the optic fibres in the owl is strong, though it has not yet been anatomically demonstrated.

In the fourth lecture he takes up the auditory centre. From his previous, as well as from fresh experiments to determine the centre for this sense, Ferrier concludes that the caudal portion of the superior temporal gyrus is the centre. Clinical evidence supports this view, but Schäfer opposes it on experimental grounds. At present the results appear irreconcilable. In discussing the location of this centre in dogs—where it occupies not the first but the second temporal gyrus—Ferrier takes occasion to suggest that what appears to be the first temporal gyrus in this animal is really the homolog of the insula in the higher forms. If this view be correct, then the position of the auditory centre is homologous in the primates and carnivora.

The centres for tactile sensibility are next discussed. As an introduction, the views concerning the paths in the spinal cord and some new experiments on monkeys are given, but all without establishing any positive conclusion. Ferrier's own observations on the disturbances of tactile sensibility after interference with the hippocampal gyrus and those of Horsley and Schäfer on similar disturbance from destruction of the gyrus fornicatus, are described in the opening of the fifth lecture, and he concludes that in the limbic lobe we have the centre for cutaneous sensibility. Thus far no evidence has been given for subdivisions of this centre, but it would appear that in each hemisphere it is connected with both sides of the body, though mainly with the opposite side.

For the olfactory and gustatory centres, both experimental and clinical evidence is scanty and the former contradictory. Relying mainly on his own experiments and the suggestions of comparative anatomy, Ferrier maintains that these centres are in hippocampal lobule and the tip of the temporal lobe.

In the sixth and last lecture the bilateral connections of certain cortical centres—such as those for the trunk—are discussed. It would appear that there is never any recovery of function due to the assumption of new functions by other parts of the cortex, but that the apparent restitution depends ultimately on this bilateral connection. It would further appear that the associated movements of limbs on opposite sides of the body are due to similar anatomical connections. The complete separateness of the motor areas and those for the dermal senses is maintained on the ground of both experimental and clinical evidence. The motor character of the cortical motor centres and their dependence on the surrounding sensory centres is emphasized. Of the function of that portion of the hemispheres lying in front of the praecentral sulcus little can be said, save that it is connected with fibres in the anterior portion of the internal capsule which degenerate downwards on its removal; that it passes over into the centres for the movements of the head and eyes, and that when it is removed both men and animals show some impairment of intelligence.

The So-called Motor Area of the Cortex. EDWARD B. LANE, M. D. American Journal of Insanity. April, 1891.

The author examines some of the evidence for the motor character of certain regions of the cortex. In pursuing this he discusses the muscle sense, aphasia in its various forms, and the very interesting cases of "motor hallucinations" described by Tamburini and Séglas. In these cases the patient detected the words which are spoken to them, or better through them, or which they are forced to speak (!) not through an auditory sensation, but by means of the "movements of their own tongue," to employ their expression. In the case of Tamburini the tongue could be seen to move at the tip, but when held motionless (?) the hallucinations still occurred. Further, while the patient is pronouncing one group of words she feels at the same time others forming in her mouth. The author concludes strongly in favor of the sensory nature of the so-called motor cortex.

In criticism of this general view a little anatomy will assist us. (Supposing that motor cells, or those giving rise to efferent impulses, exist predominantly in the motor regions, they must be started into action by impulses from the periphery—i. e., sensory impulses. One question is then whether these sensory impulses reaching the motor cortex by sensory fibres there find sensory, or better central, cells with which they connect and by way of which they act on the motor cells, or whether the sensory fibres act directly on the motor cells. Histology does not enable us to decide the point, though pending a decision the latter view has been generally accepted. That the motor region contains a very large number of cells that carry efferent impulses from the cortex, we know from the make up of the internal capsule, and the pyramidal tracts, and the question here is, whether these peripherally discharging cells have some sensory function. This has been usually answered in the negative. We do not say that these usual views are correct, but think that the detailed anatomy of the cortex as well as the clinical facts should be admitted into so important a discussion. REV.)

Hemianopsia. HENRY D. NOYES. N. Y. Medical Record. April 4, 1891.

In considering hemianopsia as "a visual manifestation of intra-cranial disorder" a number of interesting points are clearly developed. The very large number of instances in which the dividing line in hemianopsia spares the fixation point is important. This occurs in most cases not only of the homonymous form, but also in those of double hemianopsia, as illustrated by some three cases. This immunity of central vision in these cases cannot at present be adequately explained. Forster's suggestion of better blood supply to the cortical region corresponding to the fovea, is at present an hypothesis. In discussing the relation between the cortex and the retina Hun's case is quoted as significant. It must be remembered, however, that according to the experiments on animals the (ventral) lower portion of the cuneus is associated with the lower portion of the retina, while in Hun's case the lower portion of the cuneus is associated with the upper portion of the retina—an important difference. Sector defects in the field are more usually associated with disease of the cortex, while irregular defects are more likely to be subcortical.

As regards the perceptions of light, color and form it occurs, of course, that the loss of light perceptions necessarily involves the other two—but either of these alone—form or color may be lost independently. The two theories advanced to explain these are (1) separate areas for the two functions, lying beside one another, and (2) separate strata in the cortex lying above one another. It would be rash to say that either view was satisfactorily supported, but the latter seems to have rather the better support from the cases cited.

History of a case of sarcoma of genu of the corpus callosum, presenting symptoms of profound hysteria: With autopsy. CHARLES A. OLIVER, M. D. University Medical Magazine. Philadelphia, April, 1891.

The patient was a woman 43 years of age, who had suffered from severe mental strain associated with retroversion of the uterus. She exhibited symptoms shortly before her death which led to the diagnosis of profound hysteria, possibly combined with a gross intracranial lesion situated anteriorly and at the base of the brain.

The basis of this was the mutability of the ocular symptoms; the characteristic fields; the absence of any expressive motor changes; the condition of the fundus oculi, in association with the mental derangements; the loss of the senses of smell and taste; ovarian tenderness; abundant limpid urine without abnormal excreta; the absence of cephalgia, vomiting, vertigo, or any gross general symptoms of cerebral growth and a constant highly emotional condition.

In the left eye, vision was lost save in a small region to the nasal side of the visual field. Central vision for form for the right eye was but $\frac{1}{50}$th and could not be optically improved. The left pupil reacted only to the stimulation of the region mentioned. In the right eye a sluggish reaction of the pupil followed stimulation of either half of the retina. The field for color vision was very variable owing to the rapid fatigue due to the tests, the color first tested giving the largest field. The disturbance of vision was first noted by the patient, next smell and taste were lost by degrees and in the order named. Disturbance in hearing was not recognized, but upon testing, hearing was found deficient. Extreme lassitude was followed by her remaining continuously in bed. Visual illusion and hallucintions then appeared. The latter were of a very persistent sort, the former took the form of indefinite multiplication of special objects—all clearly projected. Tactile illusions followed and combined themselves with the visual ones. The muscle sense and that of pain and temperature appeared normal. There was no indications of any form of aphasia.

At the autopsy, the brain alone was examined. The tumor above mentioned was found attached to the genu. Its shape was hemispherical and its two greatest diameters six and five and one-half centimeters respectively. Its greatest bulk lay to the left of the median line. The uncinate gyri, the olfactory tubercles, the cephalic portion of the gyrus fornicatus and both optic nerves, but especially the left one, were the parts most affected.

The disturbances of vision are associated with the pressure on the optic nerves; those of taste and smell with that on the uncinate gyrus and the neighboring olfactory pathways; that of hearing—which was specially deficient on the left side—with the greater bulk of the tumor on that side; and those of touch were not specially referred, but would most naturally fall in with the pressure on the anterior portion of the gyrus fornicatus. A histological examination of the compressed structures gave negative results.

Contributions to the Pathology of infantile cerebral palsies. B. SACHS, M. D. N. Y. Medical Journal. May 2, 1891.

One purpose of this article is to point out, by careful comparison of the clinical symtoms with the pathological findings, those cases in which the surgeon may properly interfere. Another purpose is to emphasize the view that a much larger number of these palsies than has been hitherto admitted, are of cerebral origin. In the pursuit of this latter end the author is but insisting upon views which he has previously advanced.

A brief table gives the conclusions which he has reached in the cases, the morbid lesion, form of palsy, distinguishing symptoms and conditions being brought together in three groups, arranged according to time of onset as "prenatal," "birth" and "acquired palsies." Further, an account of two cases is given in detail and illustrated by three plates.

The first case is that of a boy of eight years who was well until six years and a half of age, when he was seized with convulsions and developed right hemiplegia—the face included. He was hydrocephilic and and the head was found to be still enlarging. He had had repeated epileptic seizures involving the right hand only. His disposition was happy and his mental development good, though somewhat retarded. Later, he suddenly fell, without loss of consciousness. The hemiplegia was then found complete, the sphincters not being involved. Fever developed. Vision was disturbed, the disturbance ending in blindness. Speech became difficult and stupor was followed by coma. The motor nerves of both eyes became involved later. Death at end of eight weeks. The autopsy showed the brain much enlarged and quite smooth caudad. A cyst was found in the left ventricle and in this a large tumor (gliosarcoma), filling a large portion of the distended ventricle. Another large tumor was found near the top of the right temporo-sphenoidal lobe. Both tumors pressed on the brain axis and the eye symptoms are thus explained. The motor tracts in the cord were degenerated. The cyst, occupying a large portion of the motor area and due probably to a subpial hemorrhage, is offered as the explanation of the initial hemiplegia and the tumors, as that of the subsequent and fatal attack. The hydrocephalus is not considered as important in determining the course of events.

The second case was that of a chronic meningo-encephalitis in a boy of one year, due probably to a wide spread effusion of blood between the pia and the cortex at the time of birth.

Vergleichend-anatomische Untersuchungen über den Fornix und die zu ihm in Beziehung gebrachten Gebilde im Gehirn des Menschen und der Säugethiere. Von JACOB HONEGGER. Mit 10 Lichtdruk-Tafeln. Inaug. Diss. Zürich. Genf, 1890.

This pamphlet, which to say the least is a remarkable production, appears to have been printed at the same time in the Recueil Zoölogique Susse t. V, and thus the author was assisted in publishing his 234 pages of text and ten plates, on some of the more neglected parts of the brain. He opens with 76 pages of historical introduction, which is intended to fill the gap existing between the account of Burdach and the present day. This account is very full. His material for study comprised a long series of sections from man, calf, sheep, dog, pig, cat, rabbit, mouse, and from several birds, reptiles, amphibia and bony and cartilaginous fishes, many of these animals being represented by several series in different planes and stained with gold, carmine, or Weigert's haematoxylin. So much for his base of supplies. By comparison of this rich material the author proceeds to examine critically the structure of the cornu ammonis and the fascia dentata, striae Lancisii, psalterium fornix longus and fimbria, septum pellucidum and pedunculus septi pellucidi, columnae fornicis, tuber cinereum and corpus mamillare, decussatio subthalamica posterior and pedunculus corporis mamillaris, the bundle of Vicq d'Azyr and of v. Gudden, the fasciculus longitudinalis posterior, taenia thalami optici, ganglion habenulae, pedunculi conarii, Meynert's bundle (fasiculus retroflexus), taenia semicircularis and nucleus amygdalae.

The structures are treated from the purely anatomical side so that, even if we felt capable of reviewing the results, which we confess we do not, it would hardly be possible to do so in this place.

It is a valuable paper from the detail with which many of these neglected structures are discussed and the broad comparative basis which the author has for his conclusions. It is hard reading, and to this the subject matter and the style are both contributors. The phototype plates are artificially admirable, but would be aided by outline diagrams in each case, and as it is a paper for reference rather than continuous reading, an index would be a great assistance.

The epithelium of the brain cavities. By P. A. FISH. Proc. Am. Soc. of Microscopists. 1890. 1 plate.

The author studied the living epithelium or endyma in the brain cavities of the cat, using animals that were adults, six weeks old or newborn, and found ciliated cells in all cases. At the points of intrusion of the plexuses into the cavities, as in the paracœle (lateral ventricle), the covering cells were of the pavement form and without cilia. The discrepant statements concerning the existence of cilia of the brain cavities of adult man probably depend, as suggested, on the difficulty of obtaining really fresh material. The paper is accompanied by a useful bibliography.

Ueber Störungen der kompensatorischen und spontanen Bewegungen nach Verletzung des Grosshirns. A. V. KORÁNYI and J. LOEB. Archiv f. d. ges. Phys. Bd. XLVIII. 1891.

The research in question forms a further contribution to the analysis of the motor disturbances following lesion of the cerebral hemispheres in rabbits and dogs.

The first question taken up relates to the nystagmatic movements of the eyes in a rabbit fixed in the primary position upon a holder which can be revolved about a vertical axis. The direction of the nystagmus is referred to the animal, and the slower part of the oscillation is the one always designated. Upon rotating a normal rabbit, under the conditions just indicated, the nystagmus during rotation is in the opposite sense to the rotation, but when the rotation is stopped, it occurs for a short period in the same sense. In normal rabbits the direction of rotation, whether to the left or right, has no influence on the number of oscillations which are approximately the same in both cases, both during and after rotation. The authors rotated their animals ten times, then stopped the rotation and counted the number of subsequent oscillations. These were approximately the same for rotation to right or to the left in normal rabbits. When, however, the experiment was tried with rabbits from which the occipital portion of one cerebral hemisphere (always the left hemisphere in their experiments), had been removed, it was found that the direction of the rotation made a marked difference in the number of subsequent oscillations. A rabbit from which the occipital portion of the left hemisphere had been removed gave, after rotation to the right, a much larger number of subsequent oscillations than it did after rotation to the left. So too, these rabbits compensated by movements of the body for rotation to the right better than for rotation to the left. During rotation to the left the rabbit must make nystagmatic movements to the right, and these were found to be more numerous when the rotation was to the left. In these cases then oscillations to right were the most readily obtained whether the animal was observed during or after rotation. It should be mentioned that when the frontal portion of the hemisphere was removed these differences in reaction were not observed.

When the lesion was made in the frontal portion of the brain, then compensatory movements were not affected. On the different effects of the lesion, according to its location, the authors lay no stress, but pass on to more general considerations. If after the injury to the brain there is a disturbance in compensatory movements—those of the eye being only one example—it must be due to a change in the irritability of the nervous mechanism involved in the reaction. This they think tends to favor the view of Goltz that "injury to the brain causes a decrease in the irritability of the lower centres in the spinal cord." In general they determined a greater tension in the trunk muscles on the side opposite to the lesion, but the explanation of this observation is not given.

The ear of man: its past, present and future—Lecture IX. in the Biological lectures delivered at the Marine Biological Laboratory of Wood's Holl in the summer session of 1890. Boston, Ginn & Co., 1891.

This lecture contains a general presentation of some observations on the morphology of the vertebrate ear coupled with some remarks on its physiology. The morphological portion is to appear more in detail in an early number of the Journal of Morphology. The author argues that the internal ear is derived by modification from the organs of the lateral line, and that it is to be regarded as representing two sense organs, one indicated by the utriculus and the other by the sacculus, each with a system of semicircular canals. Taking his departure from Allis' paper on the development of the lateral line organs in the fish, he shows how from the first sinking in of the auditory pit to the full development of mammalian ear, the process is parallel to that which takes place in the organs of the lateral line. When thus regarded, the Cyclostome ear—which has been a stumbling block to the comparative anatomists—appears as a simpler and less developed ear rather than an aberrant or degenerate one. The double nature of the organ is suggested by the double nerve supply—by what in the higher forms are considered the two branches of the auditory nerve—and by the fact that, considered schematically, the organ may be divided into equivalent portions, using the prolongation of the ductus endolymphaticus as an axis. If we accredit the anterior and horizontal canals to the utriculus we have the same number of groups of sensory cells as in the sacculus and its appendages. To be sure the latter has but one canal—the posterior, with its proper crista—but it also contains the *macula acustica neglecta* of Retzius, which, if the canal belonging to it had developed, would have established the numerical symmetry that the scheme demands. In speaking of the physiology the author lays much stress on the contradictions among the older authors who have investigated the semicircular canals and does not utilize the recent results like those of Delarge and Breuer, which are, if anything, more important.

Die Kopfnerven von Salamandra maculata im vorgerückten embryonalstadium untersucht. Von Baron Jos. von Plessen und Dr. Med. JOHN RABINOVICZ. Mit 2 lithographischen doppeltafeln und 4 Zinkographien im Text. München, J. F. Lehmann, 1891.

The plates in this paper are from the sections reconstructed after the method of His and are very instructive. In this salamander the trochlearis nerve is wanting; the ganglion of the fifth nerve is double; the glossopharyngeus appears to have no ganglion, but simply to pass thro' the vagus ganglion; and the hypoglossus in accordance with the observations of others has but one root, the ventral. This, however, divides into a dorsal and ventral ramus, and in the dorsal ramus a distinct ganglion is to be seen, thus restoring the hypoglossus in these forms to the type of the spinal nerves—an important observation.

On out-lying nerve cells in the mammalian spinal-cord. By CH S. SHERRINGTON. Phil. Trans. Roy. Soc., 1890. 2 plates.

The author has examined the cord in man, the monkey (Bonnet, Jew and Rhesus), and dog, using sections from the cords of the cat, lion, calf, rat, mouse, rabbit and guinea-pig for comparison. The cells in question are those which lie outside of the gray matter among the white fibres, and they are conveniently subdivided for description into ventral, lateral and dorsal groups. The cells in these several localities are described, and they appear in each case similar to the cells of that portion of the gray matter near which they lie. By far the most interesting is the dorsal cells, which in a given section are scattered from the point of entrance of the dorsal roots to the column of Clarke. There is some evidence that these cells are bipolar—as is also the case for the cells in the column of Clarke—and the suggestion is made that we may have here homologues of the spinal ganglion cells still included in the substance of the cord, a suggestion which has much in its favor. From the descriptive nature of the paper the evidence for this view cannot be abstracted with advantage.

Die Ringbänder der Nervenfaser. Mitgetheilt nach Untersuchungen von DR. JOHANSON durch JUSTUS GAULE. Centralbl. f. Physiologie, Aug., 1891. Heft 11.

The communication is preliminary to the fuller paper now in press. Its bearing may be briefly indicated as follows: If the nerve of a frog, or rabbit be hardened in Erlyk's fluid for 14 days, teased in water and stained for an hour with haematoxylin (alum .5%, Häm 20%), the axis cylinder is slightly tinged and at irregular intervals bands are darkly colored and are to be seen in the medullary sheath. This appearance it is argued is due to the presence here of some substance taking the haematoxylin stain and not to an insignificant deposit of the dye. These bands occupy the position of the well known clefts of Schmidt and Lantermann. They have a suggestion of fibres in them. Such is the appearance in May frogs. In June frogs the picture changes, and there is a clearly marked spiral fibre surrounding the nerve at these points. At this time, June, the axis cylinder of the nerves is small and shrunken. Later it assumes the full appearance found in the spring (May), frogs. This condition of the axis cylinders the authors associate with the proverbial misbehaviour of the June frogs when used for nerve-muscle work. It is also plain that this condition of the nerves occurs at the breeding season, and the influence of the reproductive process on these bands and the possibility of their being related to nuclear substances, are the aspects of the case which most interest Gaule.

The Journal of Comparative Neurology—a quarterly periodical devoted to the comparative study of the nervous system. Edited by C. L. HERRICK, Professor of Biology, etc., in the University of Cincinnati. Robert Clark & Co., Cincinnati, Ohio. Vol. I, No. 1, March; No. 2 June, 1891.

It is certainly desirable that the papers on comparative neurology should be grouped in some one publication, and the opportunity for this is offered by the new Journal. Original papers, reviews, notes on technique, bibliography and an editorial have formed the contents of the numbers thus far issued. In the first number the editor comments on some of the open questions, and in the second, as bearing on the relations of neurology to psychology, gives an historical account of the ideas on localization of function in the brain.

Running through both numbers is a laborious study of the avian brain by C. H. Turner, in which, for one thing, he tests the taxonomic value of the brain of birds, with suggestive results. In the first number the editor writes on "Illustrations of the Architecture of the Cerebellum." Under this head he presents the view that the superficial layer of the cerebellar cortex—the molecular layer—is, in part at least, derived from cells forming the walls of the recessus lateralis—a view which certainly requires more evidence to support it than is here given.

The remaining papers, three in number, are studies in comparative anatomy.

A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

By Edmund C. Sanford, Ph. D.

(*Second Paper.*)

III.—TASTE AND SMELL.

Sensations of Taste.

Apparatus. A potato and an apple; standard solutions of sweet, bitter, sour and salt; camel's-hair brushes; battery and zinc electrodes. The standard solutions should be made of two strengths, the stronger for testing the individual papillæ and the weaker for finding the least proportion tastable. The following proportions of tastable substances and water are convenient. Stronger solutions: Sugar, 40:100; Quinine, 2:100; Tartaric Acid, 5:100; Salt, saturated solution. Weaker solutions, (for which the water itself should be without taste): Sugar, 5:100; Quinine, 2:100 000; Tartaric Acid, 5:1000; Salt, 2:100. Special solutions of Sugar for Ex. 52: 20:100, 18:100, 16:100, 14:100, 12:100, 10:100.

49. Much of what is commonly called taste is really taste plus smell or touch or both. With the eyes shut and the nostrils held try to distinguish, by taste alone, between small quantities of scraped apple and potato, placed upon the tongue.

50. Distribution of the Organs of Taste. *a.* Using the weaker solutions and operating with a mirror or on another person, find out as nearly as you can in what part of the tongue the strongest sensations are produced by each. Test the tip, the sides, the back and the middle, putting on the solutions with a camel's-hair brush and rinsing the mouth as often as necessary. Try also the hard and soft palates. *b.* Dry the tongue with a handkerchief and test the individual fungiform papillæ with the stronger solutions, applying them with fine camel's-hair pencils. It will be found possible to get taste sensations from the single papillæ, though perhaps not all four from each. Rinse the mouth as needed. *c.* Test the surface of the tongue between the papillæ and observe that no taste sensations follow.

On *a* cf. Rittmeyer: Geschmacksprüfungen, Göttingen Diss. 1885. On *b* and *c* cf. Oehrwall, Untersuchungen über den Geschmackssinn, Scandinav. Archiv f. Physiol. Bd. II. 1890, pp. 1-69; see also abstract by the author in the Zeitschrift f. Psych., Bd. I. 1890, p. 141.

51. Minimal tastes. *a.* Find what is the greatest dilution of the weaker solutions in which the characteristic tastes can still be recognized. The same quantity, *e. g.*, half a teaspoonful, should be taken into the mouth at each trial and may be swallowed with advantage. Rinse

the mouth thoroughly as required. The following are the proportions given by Bailey and Nichols for male observers: Quinine, 1:390 000; Sugar, 1:199; Salt, 1:2240; for Sulphuric Acid, which they used instead of Tartaric, the proportion was 1:2080. *b*. The intensity of the sensation and the greatest dilution still tastable depend on the number of taste organs stimulated. Take a portion of one of the solutions of just tastable strength found in *a*, add an equal quantity of water and take a large mouthful of the mixture. The characteristic taste will still be perceived, perhaps more strongly than before.

On *a* cf. Bailey and Nichols, The Delicacy of the Sense of Taste, Nature, XXXVII, 1887-88, 557; and Lombroso und Ottolenghi, Die Sinne der Verbrecher, Zeitschrift für Psychologie Bd. II, 1891, pp. 346-48. Camerer, Die Grenzen der Schmeckbarkeit von Chlornatrium in wässriger Lösung, Pflüger's Archiv, II, 1869, 322. On *b* cf. Camerer, Die Methode der richtigen und falschen Fälle angewendet auf den Geschmackssinn. Zeitschrift für Biologie, XXI, 570.

52. Discriminative sensibility for taste. For a rough determination test with the solutions of sugar indicated above, taking first a small quantity of the standard 20% solution, then an equal quantity (the equality is important) of one of the weaker solutions, or first one of the weaker and then the standard, until a solution is found that is just recognizably different from the standard. Make this determination several times. The excess of sugar in the standard solution over the amount in the solution just observably weaker set in a ratio to the total percentage of sugar in the standard measures the sensibility. Some experimenters may be able to distinguish the 18% from the 20% solution; their sensibility would then be expressed by the ratio 2:20.

On such experiments as this cf. Keppler, Das Unterscheidungsvermögen des Geschmacksinnes für Concentrationsdifferenzen der schmeckbaren Körper. Pflüger's Archiv, II, 1869, 449.

53. Electrical Stimulation. *a*. Using a constant current and two zinc electrodes one above, the other under the tongue notice the sour taste at the positive pole and the alkaline at the negative.

On the sensations of taste in general cf. von Vintschgau, Physiologie des Geschmackssinne, Hermann's Handbuch der Physiologie, III, (pt. 2) pp. 145-224; Oehrwall, *op. cit.* On acid tastes and chemical composition cf. Corin, Action des acides sur le goût, Archives de Biologie, VIII, fasc. 1.

SENSATIONS OF SMELL.

Apparatus. Essence of cloves; olfactometer of Zwaardemaker; camphor gum; yellow wax; a dozen small wide mouthed bottles. The essence of cloves is made by adding one part of oil of cloves to fifteen parts of alcohol[1] and may be diluted with water, itself odorless, to make the solutions required in Ex. 54. For that experiment dilutions of the essence that will give the following proportions of oil of cloves will be found convenient: 1:50 000; 1:100 000; 1:200 000; 1:300 000; 1:400 000; 1:500 000. The olfactometer of Zwaardemaker in simple clinical form may be bought of Mechaniker Harting Bank, Utrecht, at 1.50 mk.; but its construction is so simple that it may easily be made in the laboratory. It will be most convenient if made double as shown in the accompanying cut. The instrument consists of a light wooden screen, say six inches square, provided with a handle below for easier holding. Through this screen, a little below the middle, a hole an inch and a half in diameter is bored, and fitted with a large cork. The cork in turn is pierced with two holes side by side an inch apart and of such size as to fit tightly upon the glass tubes next to be mentioned i. e. about 7 mm. The glass tubes should be long enough to leave 10 cm. free behind the screen and about 3 cm. free in front. The front ends are bent upward at right angles for insertion in the nostrils. The odorous substances used in this instrument are applied in the form of tubes that slide over the glass tubes behind the screen. The simplest and best for persons of normal keenness of smell are pieces of red rubber tubing 10 cm. long and of such bore as just to slide freely over the glass tubes (8 mm.). These pieces of rubber tubing should themselves be slipped into pieces of tight fitting glass tubing so as to prevent the spread of the odor from their outer surface. For Ex. 57 another odor tube, this time of yellow wax will be needed. This can easily be made by placing a glass tube (of the size of the air tubes used in the olfactometer) inside a tube such as is used to cover the rubber odor-tubes and filling the space between them with melted wax. The inner tube may then be warmed by running hot water through it till it can be withdrawn. The principle upon which the instrument works is this, namely, that the intensity of the odor varies directly as the surface of odorous substance exposed. When the odor-tubes are slipped onto the glass tubes of the olfactometer and pushed back until their ends are flush with those of the glass tubes, the air inhaled through the latter contains few or no odorous particles because no odorous surface is exposed. When, however, the odor-tubes are pulled a little away from the screen so that they extend over the ends of the glass tubes, they expose the odorous surface inside them to the current of air inhaled. The strength of the odor is proportional to the area exposed, or since the bore does not change, to the length of odor tube that extends beyond the glass tube. This last can be conveniently measured by a scale (*e. g.* centimeters and half centimeters) scratched on the inner glass tubes. The length of odor tube corresponding to a just observable odor will, of course, differ with different tubes, from person to person, and with the temperature, but tubes of red rubber are reported to give satisfactory results both as to original intensity and the constancy with which they keep their odor through considerable periods of time. The length of red rubber odor tube required by Zwaardemaker himself for a just observable odor at 18° C. is 7 mm. In use the upward turned end of one of the glass tubes is inserted in the forward part of the nostril and the subject draws his breath in the way most natural to him in smelling—the proportion of odorous particles is greater, however, when the current of air is slow than when it is rapid. The inside of the glass air tubes will need to be cleaned of adhering odorous particles from time to time.

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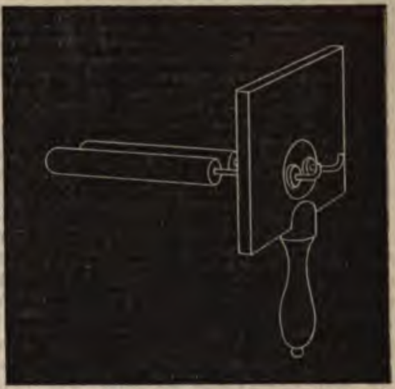
[1] Whether this essence is of the same strength as that used by Lombroso and Ottolenghi in their experiments to which reference is made below, the writer does not know.



On the olfactometer and its method of use see the following papers of Zwaardemaker: Die Bestimmung der Geruchsschärfe, Berliner Klin. Wochenschrift, XXV, 1888, No. 47, p. 950 (abstract of the same, British Med. Journal, 1888, ii, 1295); also Lancet, London, 1889, i, 1300. On an improved form adapted to liquid substances (and thus to substances of known and relatively simple chemical composition) see, Compensation von Gerüchen mittelst des Doppelriechmessers, Fortschritte der Medicin, VII, 1889, 721 ff.

54. Minimal odors. The keenness of smell may be tested with dilute solutions of odorous substances or with the olfactometer; it will be instructive to test by both methods. *a.* Tests with solutions. Pour small quantities of the solutions of oil of cloves described above into little wide-mouthed bottles, filling each to about the same height. Mark all in an inconspicuous manner. Set the bottles on a table a foot apart in a place where there is moderate circulation of air, in the order of the strength of their solutions, beginning with the water and following with the weakest solution and so on. Require the subject to smell of the bottles in succession without lifting them from the table, beginning with the water, and to indicate that in which he first recognizes a characteristic odor. If the solutions stand for any length of time where they are subject to evaporation it will be safer to prepare fresh ones before undertaking a new test. Other precautions will suggest themselves, as for example, the use of bottles of the same size and shape, and care in filling them that some of the solution is not left clinging near the mouth. The just observable solution will probably be found to lie between the 1:100 000 and 1:400 000. *b.* Tests with the olfactometer. Test the sides of the nose separately. Push the odor-tube on till its end is flush with that of the glass tube, insert the bent end of the latter into the nostril as described above, and gradually lengthen the exposed surface of the odor-tube till its odor is just discernable. Note in millimeters the length exposed.

On *a* cf. Bailey and Nichols, The Sense of Smell, Nature, XXXV, 1886-87, 74; Lombroso and Ottolenghi, *op. cit.* under Ex. 51. On *b* cf. Zwaardemaker, Sur la norme de l'acuité olfactive (olfactie), Archives Néerlandaises, T. XXV, pp. 131-148.

55. Discriminative sensibility for odors. Using the double olfactometer with both odor-tubes drawn out far enough to give an unmistakable odor, but not too strong a one, say both drawn out 5 cm., find how far one or the other must be drawn out (or pushed back) to make the odor which it gives just observably stronger (or weaker) than that of the other. The test should be made with one side of the nose only, (there is frequently a difference in sensitiveness between the two sides, due to mechanical obstruction or other cause) unless for some reason a bilateral form of experiment is desirable. Try a number of times, but be careful to avoid fatigue.

56. Fatigue of smell. *a.* Hold a piece of camphor gum to the nose, and smell of it continuously, breathing in through the nose and out through the mouth, for five or ten minutes. A very marked decrease in the intensity of the sensation will be observed, reaching perhaps even to complete loss of the odor. *b.* It is important, however, to observe that fatigue for one substance does not cause obtuseness for all other substances, though it does for some. Smell of some essence of cloves and of some yellow wax, then fatigue for camphor as in *a* and smell of the essence of cloves and of the wax again. The odor of the wax will probably be fainter, that of the essence of cloves unaffected.

Cf. Aronsohn, Experimentelle Untersuchungen zur Physiologie des Geruchs, DuBois-Reymond's Archiv, 1886, pp. 321-57.

57. Compensation of odors. *a.* Experiment with the olfactometer on one side of the nose as follows. Hold against the end of the rubber odor-tube another odor-tube of wax, partly covered on the inside by a glass tube of the same size as that used in the olfactometer, in such a way that the air must pass through both to reach the nose. Then gradually increase the length of the rubber tube exposed till the odor of the

wax is no longer perceived. If the experiment is carefully performed a point may be found where the two odors nearly compensate and the resulting sensation approaches zero. If the rubber is lengthened beyond this point its odor overpowers that of the wax; if it is shortened it is overpowered by that of the wax. A *mixture* of the odors is hardly to be found. *b*. Repeat the experiment, using the double olfactometer with rubber on one tube and wax on the other. The compensation will be observed as before though each side of the nose receives a separate stimulus. If the two sides of the nose are not equally keen scented the proportions of the tubes that give compensating odors will not be the same as before. Care should of course be taken to avoid fatigue.

Cf. Zwaardemaker, Compensation von Gerüchen mittelst des Doppelreichmessers. Fortschritte der Medicin, VII, 1889, No. 19. pp. 721-731.

On smell in general, beside the literature already cited cf. von Vintschgau, Physiologie des Geruchssinnes, Hermann's Handbuch der Physiologie, III, (pt. 2) pp. 225-286, and the literature there cited.

IV.—HEARING.

SOUNDS IN GENERAL.

Apparatus. A watch, 2 yards of three-eights inch rubber tubing, 2 tuning-forks (the ordinary A forks sold at music stores at 25 cents each will answer if a couple are chosen that prolong their sound well), and a hammer. A watch is not an ideal instrument for testing acuteness of hearing, but has the advantage of ready accessibility and simplicity in use.

For other special instruments for testing acuteness of hearing cf. Hensen, Physiologie des Gehörs, Hermann's Handbuch der Physiol. III. pt. 2, pp. 119-120 and the references there given; also Jacobson, Ueber Hörprüfung und über ein neues Verfahren zur exacten Bestimmung der Hörschwelle mit Hülfe elektrischer Ströme, Du Bois-Reymond's Archiv, 1885, 189. For apparatus for testing the discriminative sensibility for sounds cf. (for noise) Starke, Die Messung von Schallstärken Wundt's Philos. Studien, III, 1886, 266, and Zum Mass der Schallstärke *Ibid*, V, 1889, 157; (for tone) Wien, Ueber die Messung der Tonstärke, Inaug. Diss. Berlin, 1888, also in Wiedemann's Annalen, XXXVI, 1889, 834—857.

On hearing in general cf, Helmholtz, Sensations of Tone, Eng. tr. by Ellis, Hensen, *op. cit.*; Stumpf, Tonpsychologie; Wundt, Physiologische Psychologie.

58. **Minimal sounds.** *a*. Experiment in a large room, furnished (to lessen the echoes) and as free as possible from noise. Let the subject be seated with his side toward the experimenter, his eyes closed and his ear upon the other side plugged with cotton. Let the experimenter then find what is the greatest distance at which the subject can still hear the tick of a watch held at the level of his ear and on the prolongation of the line joining the two. This is easily done with sufficient accuracy by drawing a chalk line on the floor, marking off feet or meters and fractions upon it and estimating by eye the point of the line directly under the watch. Try several times for each ear both when the watch is being brought toward the ear and when it is being carried away. The experimenter should from time to time cover the watch with his hand to discover whether or not the subject really hears or is under illusion. For normal ears the distance found may vary from 2.5 m. to 4.5 m. and may even rise to as much as 9 m. *b*. The subject should notice in this experiment the very marked intermittences of the sound when just upon the limit of audibility. It will for a few seconds be heard above doubt and a few seconds later will as certainly not be heard.

On *a* cf. von Bezold, Schuluntersuchungen über das kindliche Gehörorgan, Zeitsch, f, Ohrenheilkunde, XIV, 1884-85, and XV. 1885-86. This paper gives the results of numerous tests on Munich school children, not only with the watch but also with the acoumeter of Politzer and with whispered speech. On *b* cf. Urbantschitsch, Ueber eine Eigentümlichkeit der Schallempfindungen geringster Intensität, Centralblatt f. d. medic. Wissensch., 1875, 625; N. Lange, Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, Wundt's Philos. Studien, IV, 1888, 390; Münsterberg. Schwankungen der Aufmerksamkeit, Beiträge zur experimentellen Psychologie, Heft 2, 1889, 69.

59. **Auditory fatigue.** Cause an assistant to strike once with a hammer on the floor, or to clap his hands. With the ears open a single

sound, or at most a single sound and transient echoes are heard. If, however, the ears are kept closed with the fingers till half a second or more after the stroke (the time may easily be fixed by rapid counting), the fainter echoes will be heard like a new stroke. In the first case, fatigue from the original sound deadens the ears to the fainter echoes, though they may still be heard by attentive listening; in the second case they are more strongly heard because the closed ears are unfatigued. The sound produced by the simple opening of the ears without any objective stroke will be less if the finger is not put into the ears, but presses the *tragus* back upon the opening. *b*. Insert in the openings of the ears the ends of a rubber tube. Strike a tuning-fork and set it upon the tube at such a point that it sounds equally intense to the two ears. (The sound will then probably appear to be located in the head midway between the ears—at least not nearer one than the other). After a few seconds strike the tuning-fork again, pinch the tube on one side, say the left, so as to shut off the sound from the ear on that side, set the tuning-fork on the tube and keep it there till the sound has become rather faint. Then allow the pinched tube to open and notice that the sound is now stronger on the left than the right and apparently located on the left. Cf. later experiments on the location of tones.

On *a* cf. Mach, *op. cit.* (under Ex. 39) p. 58. On *b* cf. Urbantschitsch, Zur Lehre von der Schallempfindung, Pflüger's Archiv, XXIV, 1881, 574-579 and references there given. See also Stumpf, Tonpsychologie, I, 360-363, where other instances of fatigue are cited.

60. Inertia of the auditory apparatus. *a*. Inertia tending to keep the auditory apparatus out of function can be demonstrated as follows. Place the ends of a rubber tube in the ears and set upon the middle of it a low tuning-fork sounding as faintly as possible. Notice that the sound does not reach its maximum intensity for an appreciable length of time; if the fork is barely audible this may be as much as a second or two. Be careful not to increase the pressure of the fork upon the tube after first setting it on; for that will produce an objective strengthening of the tone; and allow an interval of several seconds between the tests so that the auditory apparatus may again come completely to rest. A tuning-fork that will preserve these minimal vibrations for some seconds and complete freedom from distracting noises will be found necessary for success.

Cf. Urbantschitsch, Ueber das An- und Abklingen acustischer Empfindungen, Pflüger's Archiv, XXV, 1881, 323.

61. Noise. Whether or not there is a distinctive sensation of noise different from that of a mass of short, dissonant and irregularly changing tones is yet under debate, with something of the weight of authority in favor of such a sensation. A little attention to the noises constantly occurring, especially to their pitch, will easily convince the observer that a tonal element is present. This is striking when resonators (cf. notes on apparatus for simultaneous tones) are used, for they pick out and prolong somewhat the tones to which they correspond, but they are not indispensable. On the other hand, attention to musical tones will often discover the presence of accompanying noises.

Cf. Wundt, Physiologische Psychologie, I, 420; Stumpf, Tonpsychologie, II, 497-514; also Brücke *op. cit. sub* 69; Exner, Pflüger's Archiv XIII, 288 ff.; Mach, Analyse der Empfundungen, Jena, 1886, 117.

62. Silence. When circumstances promise absence of external sounds, notice that many are still present and distinct, though faintly heard. Notice also the pitch and changing character of the subjective sounds to be heard. Our nearest approach to the sensation of absolute stillness is this mass of faint inner and outer sensations.

Cf. Preyer, Ueber die Empfindung der Stille, Sammlung physiologischer Abhandlungen, Jena, 1877, pp. 67-72. This section on Silence is a portion of Preyer's study, Ueber die Grenzen der Tonwahrnehmung.

SINGLE AND SUCCESSIVE TONES.

Though musical terms are occasionally used in these experiments and some discrimination of tones is necessary, it is believed that nothing is required beyond the average ability of the unmusical.

Apparatus. The upper limit of pitch may be tested with the disk siren, with tuning-forks, with steel cylinders and with little whistles of adjustable length. The last two instruments are most commonly used and may be had from almost any dealer in physical apparatus, the cylinders from $10 upward, the whistle (designed by Galton), at about $5.00. The Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, Eng., makes the whistle in two patterns, a simple one at £1. 5s and a more elaborate one at £6. R. König, 27 Quai d'Anjou, Paris, also makes two kinds, one at 12 the other at 20 fr., the latter probably a better instrument than that of the Cambridge Scientific Instrument Co. at £1. 5s. For description and prices of the more expensive kinds of apparatus consult König's catalogue.

For testing the lower limit of pitch large tuning-forks may be used or the difference tones of small tuning-forks or of stopped organ pipes. Large tuning-forks could probably be made well enough for demonstrative purposes by almost any blacksmith and their pitch determined approximately by making them record their vibrations graphically upon a piece of smoked glass for ten seconds. The large fork with sliding weights (24-16 double vibrations per sec.) manufactured for this purpose by König costs 300 fr. and his set of high pitched forks for the difference tone method costs 340 fr.

For Ex. 65 and others that follow, almost any musical instrument of a considerable range of pitch will answer.

For Ex. 65c. use a piston whistle such as is sold in toy stores at five cents. Two that are in the laboratory here reach the proper pitch when their pistons are pushed in as far as possible. It would be easy, if still higher tones were needed, to so alter the whistles as to make these possible.

For Ex. 67 prepare a series of forks each differing from the next by about three vibrations a second. Half a dozen C forks such as are sold at the music stores will answer, if those are chosen that prolong their sound well. Take one of them as a standard and make the next sharper by filing a little at the free end of the prongs till it beats (cf. Ex. 75) with the standard three times in a second. Count for 10 seconds, if possible, and divide the total count by 10 to find the rate per sec., counting the first beat nought. (For precautions to be used in attempting an accurate count cf. Helmholtz, *op. cit.* 443 where Ellis gives all necessary particulars.) Having brought this fork approximately to three beats per sec. take it as a standard and make the next fork three beats sharper than it in the same way and so on. Make also a series of forks differing by three vibrations each, which shall be flatter than the normal C. It will be well to make a recount for greater accuracy after the forks have had a chance to cool. In order to flatten the forks a little as mentioned in Ex. 67 little riders of rubber tubing may be placed upon the prongs. Make these riders by cutting off quarter inch bits from tubing that will fit tightly upon the prongs of the forks. For the resonance bottles mentioned in the same experiment take a four ounce wide mouthed bottle and tune it to the forks by gradually closing its mouth with a bit of glass (*e. g.* a microscope slide). When the amount of closure is found which gives the greatest intensity of sound, fix the glass in position with wax. (For picture and description of such bottles see Meyer, *Sound*, pp.102-103). A standard instrument for giving such small differences of pitch as are represented above by the sharpened forks is made by Anton Appunn of Hanau a. M. under the name of a *Tonmesser*. (See picture and description in Wundt's Physiologische Psychologie, I, 431 f.) and costs for the complete instrument, with pitches ranging from 32 to 1024 vibrations per sec. and a blowing table, 1060 marks. Single octaves without the blowing apparatus range in price from 150 to 320 marks. The same maker also offers a set of forks giving a series of tones differing each from the next by a small fraction of a vibration (*Tondifferenz-apparat*) at 96 marks. See his price list also for other apparatus useful in the experiments of this section.

For Ex. 69 a hydrogen generator (cf. an elementary chemistry) will be necessary. In blowing the hydrogen and air bubbles it will be found convenient to have the mixed gases in one large bottle and to force them out by pouring in water.

For Ex. 70 a pint bottle.

Some of the apparatus suggested at the beginning of the next section will also be found useful in this.

63. Highest tones. With the apparatus at hand for the purpose, find what is the highest audible tone; i. e., if the cylinders are used, the shortest cylinder which still gives a ringing sound on the stroke of the hammer, or if the whistle is used, the closest position of the plunger at which a tone can still be heard beside the rush of air. If a number of persons are tested it is not improbable that some will yet hear the tone after it has become inaudible for the rest.

64. Lowest tones. *a.* If low pitched tuning-forks are at hand, find what is the slowest rate of vibration that can yet be perceived as a tone. In some physiological laboratories electric tuning-forks or interrupters are at hand which have vibration rates of 25 per second. Low tones can be heard from these, though they have many overtones. The latter can be partly damped by touching the tines mid-way of their length with the finger and partly avoided by bringing the ear not to the free end but to a point somewhat further toward the handle. The determination of the lower limit of audible pitch is difficult and uncertain because of the great difficulty which observers, even those of trained ear, find in distinguishing these lowest tones from their next higher octaves. *b.* The general character of these deep tones can be demonstrated with sufficient clearness upon the contra octave (C_1–C) of a church organ if one is accessible and tuning forks are lacking.

65. Some characteristics of high and low tones. *a.* High tones are smoother than low tones. This is clear with almost all tones used in music, and particularly so with those of reed instruments. It is largely due to the beating of the partial tones (see Exs. 82 ff. and 75 ff.) among themselves and even with the fundamental tones. Play the scale of the instrument at hand from the lowest to the highest, or sing the ascending scale. The difference of roughness is observable also with simple tones, but only at lower pitches and is even there less marked. *b.* High tones except the very high, produce a more intense sensation in proportion to their physical intensity than do low tones. Strike a low tuning-fork in which the over-tones are to be heard and notice that the over-tones can be heard at a greater distance than the proper tone of the fork. *c.* Some high tones are particularly strengthened by the resonance of the outer passage of the ear. These generally lie between c^4 and c^5 and give to the tones of this octave a superior strength, and ear-piercing quality. They may be demonstrated easily with a small piston whistle like that mentioned above. Find by adjustment of the piston the point at which the tone is most piercing. Insert in the outer ends of the ear passages bits of rubber tubing half an inch long (which will change the resonance of the passage, making them responsive to a lower tone) and sound the whistle again. The piercing quality will be gone and the tone appear decidedly weaker. Remove the bits of tubing and sound the whistle as before; the original quality and intensity reappear.

d. Very closely associated with the pure tonal sensations are certain of a spatial quality. Compare in this respect the sensations of the tones observed in Ex. *c* above, or better still those of Ex. 63, with those of Ex. 64 or any other deep tones. Play the scale through the complete compass of any instrument, keeping this quality in mind. *e.* The emotional shading of tones changes with their pitch. Recall the descriptive terms used: Deep, low, tuneful, sharp, acute. Play the scale and judge of the appropriateness of these terms to match the shades of feeling that mark the tones of low, middle and high pitch, distinguishing those that refer to pitch from those enumerated in Ex. 86 which refer to timbre.

Cf. Stumpf. Tonpsychologie, I, 202-218, II, 56-59, 514 ff.; also Mach, *op. cit.* under 61, p. 120 ff. On *c* and *e* cf. Helmholtz, Sensations of Tone (2nd Eng. ed.) p. 179 and p. 69. On *d* cf. James, Psychology, II, 134 ff.

66. Recognition of absolute pitch. This experiment can, of course, give accurate results only with those of very decided musical ear and skill, but it may be tried with any subject that knows the names of the notes. *a.* Strike various notes in different parts of the scale of the instrument and require the subject to name the note given. Record the note struck and the subject's answer. He should be seated with his back toward the experimenter or should keep his eyes closed.

Cf. Stumpf, *op. cit.* I, 305-313, also II, index, *Höhenurteile*, for experiments on trained musicians.

67. Just observable difference in pitch. *a.* Test as follows with the set of mistuned forks mentioned above. Let the subject pick out from the mistuned forks that which sounds to him most like the normal fork, striking and holding them successively (never simultaneously) over a resonance bottle. If all of them seem more than just observably different let him put the riders (described above) on the one that is next higher and gradually lower the pitch by sliding them toward the ends of the fork till the two, heard successively, are just different and no more. The experimenter may then determine the error of the subject in vibrations per second approximately by counting the number of beats produced by the forks when sounded together. If the number of beats per second is less than 2 or more than 6 it will be best to get the difference in pitch with some other of the forks first, so as to avoid too slow or too rapid counting, and from that to arrive at the difference from the standard fork. Repeat the test several times and average the result, but take care to avoid fatigue. This experiment will not be refined enough for testing those of keen musical ear.

Cf. Preyer, Grenzen der Tonwahrnehmung, Sammlung physiologischer Abhandlungen, I, (Jena, 1877) 26 ff.; Stumpf. *op. cit.* I, 296-305; Luft. Wundt's Philos. Studien, IV, 514.

68. Differences in pitch that are just recognizable as higher or lower. It is easier to recognize a difference than to tell its direction. *a.* Experiment as in 67*a*, but require the subject this time to pick out and adjust a fork that is just observably sharper or flatter than the standard.

On *a* cf. Preyer, *op. cit.* 28, 36. For experiments on extremely unmusical subjects cf. Stumpf, *op. cit.* I, 313-335.

69. Number of vibrations necessary to produce a sensation of pitch. Arrange an apparatus for blowing soap-bubbles with a mixture of hydrogen and air. Blow bubbles of different sizes and touch them off with a match, either in the air, or, if proper precaution is taken to prevent the ignition of the mixed gases in the vessel and any resonance in the pipe, while still hanging. The explosion of these bubbles is supposed to produce a single sound wave. The pitch of the sounds produced cannot be accurately given, but the report of the large bubbles is distinctly deeper than that of the small ones.

Cf. Brücke, Ueber die Wahrnehmung der Geräusche, Wien. Sitzb., 3te Abth., XC, 1884, 199-230.

70. The apparent pitch of tones is affected by their timbre, tones of dull and soft character regularly seeming lower in pitch than those that are brighter and more incisive. Require the subject to pick out on some stringed or reed instrument the tone corresponding to that produced by blowing across the mouth of a medium sized bottle. Too low a note will generally be chosen, at least by those without special musical training. The tones should be sounded successively, not at the same time, during the test. Afterward they may be sounded together and the pitch of the bottle determined approximately by finding with which tone of the instrument its tone makes the slowest beats (cf. Ex. 75). It should be remembered, however, that it will be possible to get beats also with tones an octave lower and an octave higher than that corresponding most nearly with the true pitch of the bottle tone. *b*. Repeat the experiment, taking the pitch of the bottle first with the voice and then finding the tone on the instrument corresponding to that sung. The illusion will probably disappear when the test is thus made.

Cf. Stumpf, *op. cit.* I, 176, 227-247, especially, 235-245.

71. Recognition of musical intervals. Cause a familiar air to be played, first in the octave of *c* and then in that of c'' in the same or another key. Even those of no musical training will easily recognize that the air (i. e. the succession of musical intervals in fixed rhythmical relations) is the same in both cases; and any mistake or variation will be noticed as easily as if the air had been repeated at the first pitch. The power of recognizing intervals is very much more highly developed in persons of musical training, but any one that can whistle a tune at one pitch and repeat it recognizably at another undoubtedly has the rudiments of it.

For exact methods of testing the accuracy of the power of recognizing intervals cf. Preyer, Ueber die Grenzen der Tonwahrnehmung, Jena, 1876, pp. 38-64; and Schischmánow, Untersuchungen über die Empfindlichkeit des Intervallsinnes, Wundt's Philos. Studien, V, 558-600 and the references there given.

72. Pitch distances. Beside the interval relations of tones, and overshadowed by them in musicians, are certain relations of separateness or distinctness or distance in pitch, which do not depend on the ratios of vibration rates. Equal musical intervals (i. e. intervals between tones that have vibration rates in a fixed ratio to each other, e. g. *C D* and c'' d'') do not correspond to equal pitch distances. Sound the half tone interval *cc*–sharp through the range of the instrument, beginning in the bass and ascending. Notice the increasing distinctness and separation of the tones as the interval is taken higher and higher. For the very highest tones there is probably a decrease of separateness again. The difference is most striking, however, with intervals smaller than those in common use, e. g. with quarter or eighth-tones. On the harmonical (cf. notes on apparatus at the beginning of the next section) strike in succession the *c-sharp* and *d* keys in the four lower octaves beginning with the lowest. In this instrument the *c-sharp* key is given to another *d*, a comma, or about one-ninth of a tone, flatter than the regular *d* of the scale.

Cf. Stumpf, *op. cit.* I, 249-253; Lorenz, Untersuchungen über die Auffassung von Tondistanzen, Wundt's Philos. Studien, VI, 1890, 26-103; also a prolonged discussion between Wundt and Stumpf (and Engel) in succeeding numbers of the Studien and in the Zeitschrift für Psychologie. Helmholtz, *op. cit.* pp. 264-65, 285, 287.

73. The effect of a given tone in a melody depends in part on the succession of tones in which it stands. Cause a simple air, in which the same tone recurs in different successions of tones, to be played and notice the difference in effect in the different circumstances.

Mach, *op. cit.* under 61, p. 130-131.

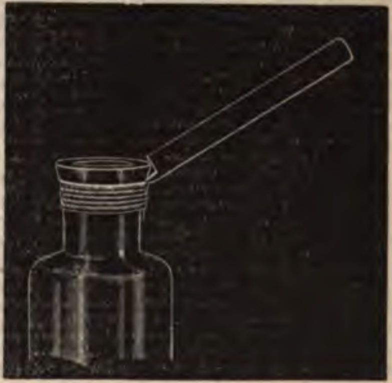
74. Tones that vary irregularly in time and in pitch are unpleasant. Test with a piston whistle.

SIMULTANEOUS TONES.

Apparatus. For the experiments of this section access to some large musical instrument is essential, and a reed instrument is to be preferred to a piano if only one is to be used. A parlor organ will answer in most cases, but sometimes the specially tuned Harmonical designed by Ellis to illustrate the theories of Helmholtz (see description of the instrument in his translation of Helmholtz's Sensations of Tone, pp. 466-469, also 17, 22 and 168), would be better. This instrument is made as an aid to science by Messrs. Moore and Moore, 104-105 Bishopsgate St., Within, London, E. C., at the very low price of from £8-5 to £10. For the proper tuning of the instrument, however, a set of 19 forks is necessary costing £3-10 extra. In many of the experiments a sonometer can take the place of a piano. A sonometer is simply a long flat box with a very thin top which serves as a sounding board for the strings that are stretched over it. One may be had from any physical instrument dealer at from $10.00 upward, or can be made by a carpenter. For directions for making and dimensions see Mayer, Sound, (Appleton & Co.) pp. 129-130.

For more perfect apparatus for the study of beats, difference tones, compound tones, timbre, etc., consult the catalogues of König, whose address has already been given, and of Anton Appunn, Nürnbergerstrasse 12, Hanau, a. M., Germany. Both make resonators, those in spherical form made by König are best and most expensive. A series of 10 corresponding to the first ten partial tones of *c* (128 vibrations) costs 110 fr.; a series of 19, 170 fr. Appunn's in conical form cost from 27 mk. for a set of 9 to 80 mk. for a set of 29.

The bottle whistles mentioned in Ex. 75 are easily made by fitting a piece of rubber tubing to the lip and neck of a bottle as in the cut, or better still, by slitting the tube a little way so that half the tube may extend an eighth or three-sixteenths of an inch over the lip, but care must be taken that it does not project too far.



A pair of octave tuning forks will also be needed. The large forks on resonance cases (to be had of any physical instrument dealer at a cost of from $5.00 to $20.00 according to pitch) are much to be preferred, because they sound longer after once being struck, but are not indispensable. A pair of octave forks can be made from an *a′* and a *c″* fork by filing the *a′* till it gives *c′*. Choose an *a′* fork with thick and heavy prongs and file it in the crotch and along the lower half of the prongs inside, distributing the filing so as to leave the prongs of equal thickness, till it begins to beat with *c″* when both are struck and have their stems pressed against the table. Then continue the filing carefully till the beats can no longer be heard. The filing warms the fork and makes it a little flatter than when cold; this of course must be taken into account. To make a *c‴* fork, if one is desired, a *c″* should be used and the cutting must be at the free end of the prongs. In one made here about three-quarters of an inch was taken off. The tuning is as before by filing until the beats with *c″* are first heard, then grow slower and finally disappear. In the same way an *a″* may be made as the octave of *a′*, but these small forks do not vibrate very long.

75. Beats. When tones not too greatly different in pitch are sounded at the same time they mutually interfere and make the total sensation at one instant more intense and the next instant less intense. This regular variation in intensity is called "beating." Exs. 67 and 70, where beats have been used incidentally, are a sufficient introduction to them. *a.* The rapidity of beats depends on the difference in the vibration rates of the beating tones. Prepare two bottle whistles of the same size and blow both at the same time. Slow beats will probably be heard. If not, pour a little water into one bottle (thus raising the pitch of its tone) and blow as before. Continue adding water a little at a time till the beats lose themselves in the general roughness of the tone. Blow the bottles separately now and then to observe the increasing difference in pitch. The same may be shown with a couple of piston whistles, if they are first adjusted to unison and then the piston of one or the other is slowly pushed in or pulled out. *b.* The rate at which the roughness of rapid beats disappears, as also the rate which produces the greatest roughness, differs with the pitch of the beating tones. Sound the following pairs of tones which have somewhat near the same difference in vibration rates per sec., namely, 33; and observe that the roughness from the beats decreases and finally disappears entirely at about the fourth pair: *b' c''*, *c' d'*, *e g*, *c e*, *G c*, *C G*. The *a'* and *c''* tuning forks give a vanish of roughness, representing a rate of 80-88 per sec.

Cf. Helmholtz, *op. cit.*, pp. 159-173; Stumpf. II, 449-497, especially 461-465.

76. Beats betray the presence of very faint tones both because the total stimulus is actually stronger in the phase of increased intensity and because intermittent sensations are themselves more effective than continuous ones. *a.* Strike a pair of beating tuning-forks and hold one at such a distance from the ear that it is very faint or quite inaudible. Then bring the other fork gradually toward the ear and notice the unmistakable beats. *b.* Strike a tuning-fork and hold it at a distance as in *a*, being careful to have the fork sidewise or edgewise, not cornering, toward the ear. Rotate the fork one way and the other about its long axis and observe the greater distinctness of the tone, due in this case simply to its intermittence.

77. Beats are in general attributed to the tone that is attended to; in the absence of otherwise determining causes, to the louder tone, if there is a difference in intensity, to the lower tone, or to the whole mass of an unanalyzed compound tone (see introduction to Ex. 82). *a.* Set two properly tuned resonance bottles about a foot apart on the table. Strike two forks that beat and hold them over the bottles. While both are about equally intense it is easy by mere direction of the attention to make the beats shift from one to the other. *b.* Turn one of the forks about an eighth of a turn about its long axis, which will weaken its tone and observe that the beats seem to come from the other fork. By moving first one fork and then the other the location of the beats may again be made to shift at pleasure. *c.* Warm the *c'* fork in any convenient way, (holding it clasped in the hand will do.) This will flatten it somewhat. Strike it and the *c''* fork and press the stems of both on the table at the same time, or better on the sounding board of the sonometer. Observe that the beats seem to come from *c'* fork unless it is very faint. *d.* Tune a string of the sonometer so that its third partial (or corresponding harmonic) beats slowly with the *c''* fork. (On partials and harmonics cf. Exs. 82-85.) Strike the tuning-fork and hold it over a resonance bottle, or press its stem against the table at arm's length from the string. Then pluck the string and attend to its tone, the beats may seem to affect the whole compound tone of the string. But this will not happen if the tone of the string is analyzed or if the attention is directed to the fork. The same may be tried on the piano

by picking out from the mistuned *c''* forks one that beats slowly with *c''* on the piano. Strike the *f* key and hold it down; strike the fork and observe the beats as before.

Cf. Stumpf, *op. cit.* II, pp. 489-497.

78. Difference tones.[1] When two tones are loudly sounded at the same time there results (probably from supplementary vibration of the tympanic membrane and ear bones) a third tone of a pitch represented by the difference of the vibration rates of the two original or generating tones. These difference tones are easy to hear when they lie considerably deeper than the generators, when the latter are loud and sustained, and when they make a consonant interval, though the latter is not essential. A loud difference tone may itself take the part of a generator and produce yet another difference tone—a difference tone of the second order—and so on, though difference tones of higher orders are heard with difficulty even by skilled observers of trained ear. Difference tones are hard to hear on the piano and similar stringed instruments, because of the rapid decline in the strength of the generators. *a.* Repeat Ex. 75 *a* continuing to pour water into one of the bottles till the difference tone appears. At first the roughness of the beats and the difference tone may both be heard at once. Try the same with the piston whistles, first setting them at unison and then slowly pushing the piston of one in or out while blowing hard. The beats will almost immediately give place to a low difference tone which may be heard ascending through several octaves before becoming indistinguishable from the generators. The double warning whistles used by bicyclists give a fine difference tone, to which indeed they owe their deep and locomotive like quality. *b.* Difference tones are strong on reed instruments. Press the adjacent white keys of a parlor organ, or the harmonical, by twos, beginning at *c* and going up a couple of octaves. If there is difficulty in hearing the difference tone, sound the upper tone intermittently and listen for the difference tone at the instant of pressing the key. *c.* Sound *c''* and *d''* which should give *C* for a difference tone (594—528=66). Sound also *d''* and *e''* which should give the same (660—594=66). If, however, the tuning is inexact, as it is intentionally in the tempered tuning of keyed instruments, these difference tones will be somewhat different and may be heard to beat with one each other when *c''*, *d''* and *e''* are sounded at once. Notice that you do not get these beats when the tones are sounded in pairs. On the harmonical this difference may be brought about by sounding one of the tones flat by pressing its key only a little way down. The same thing may be shown with three piston whistles blown at once, by a little careful adjustment of the pistons. *d.* The location of difference tones. The location of these tones is sometimes influenced by the location of their generators, but under favorable circumstances they seem to arise in the ears or even in the head. This is strikingly the case, both for the blower and the listeners, with the difference tones produced with the piston whistles.

Cf. Helmholtz *op. cit.*, pp. 152-159; Stumpf, *op. cit.*, II, pp. 243-257. König, Quelques expériences d'acoustique, Paris, 1882.

79. Blending of tones. The degree to which tones blend with one another differs with the interval relation of the tones taken. It is, according to Stumpf, greatest with the octave, less with the fifth, less again with the fourth, slight with the thirds and sixths and least of all with the remaining intervals. Try on the instrument the extent to which the tones forming these intervals blend, also those forming intervals greater than an octave: double octave, twelfth, etc. *b.* The blending in

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[1] König distinguishes between "difference tones" and "beat tones." Both tones, however, generally have the same pitch and the older term for them has here been retained; strictly speaking, however, the "difference tones" heard in these experiments are "beat tones."

case of the octave is so complete under favorable circumstances as to escape the analysis of trained ears. Use two tuning-forks, one an octave higher than the other, on resonance cases or held over resonance bottles. Sound the forks, first the higher, then the lower. For a while the higher fork will be heard sounding in its proper tone, but by degrees it will become completely lost in the lower and a subject with closed eyes will be unable to say whether or not it yet sounds. Stop the lower fork or remove it from its resonance bottle and notice that the higher is still sounding. Notice the change in timbre (cf. Ex. 86) produced by the stopping of the higher fork—something like the change from the vowel Ō to the vowel Ū (oo).

On *a* cf. Stumpf, *op. cit.* II, pp. 127-218, especially 135-142, 353; for his experiments on the unmusical confirming his grades of blending cf. 142-173. On *b* cf. Stumpf, *op. cit.* II, pp. 352-356, and Helmholtz, *op. cit.* pp. 60-61.

80. Analysis of groups of simultaneous tones. Ease of analysis depends on a number of conditions, among others on the following. *a.* Analysis is easier for tones far distant in the scale. Compare the ease of recognizing the sound of the c'' fork when c' and c'' are sounded together, with that of recognizing c''' when sounded with c'. Compare also the ease of distinguishing c' and a' with that of distinguishing c' and a''. *b.* Analysis is made easier by loudness in the tone to be separated. Repeat Ex. 79 *b* sounding the c' faintly the c'' strongly. No difficulty will be found in keeping the latter distinct. *c.* Analysis is easier when the tones make intervals with little tendency to blend. Compare the ease of analysis of c' c'' and c' a' or a' c''. Also notice that the addition of d'' (octave of d', fifth of g', fourth below g'') to the chord g d' g' g'' produces a less striking change than the addition of b' (major third of g', minor sixth below g''). *d.* Analysis is easier with sustained than with short chords. Repeat the last experiment making the chords very short and notice that the difference made by inserting either d'' or b' is less marked. Cf. also Ex. 95.

Cf. Stumpf, *op. cit.* II, 318-361, also his experiments, 362-382.

81. The lower tone of a chord fixes the apparent pitch of the whole. *a.* Repeat Ex. 79 *b* and notice that when the c' fork is stopped the tone appears to jump upward an octave in pitch (i. e. it takes the pitch of the c'' still sounding); but when the c'' fork is removed the quality of the tone is changed but not its pitch. *b.* Strike the chord C c'' e'' g'' or G e' g' c'' and compare the effect upon the pitch of the whole mass of tone produced by omitting C or G alone with that of omitting any one or all three of the higher tones. See also the function of the lowest partial of a compound tone in fixing the pitch, noticed below.

Cf. Stumpf, *op. cit.* II, 383-392.

Compound tones which on casual hearing seem single tones but in reality are chords deserve special attention. The tone given by the C string of a piano is made up of at least C, c, g, c', e' and g' and generally other tones. The lowest tone of the group gives the pitch attributed to the whole and is known as the *fundamental*, the other tones as *over-tones*. In another way of naming them, the component tones are all *partial tones* or *partials*, the fundamental being called the *first* or *prime partial*, the next higher the *second partial* and so on. It should be observed that the *first* over-tone is the *second* partial tone, the *second* over-tone the *third* partial, and in general that the same tone receives as a partial tone a number one higher than as an over-tone. The vibration rates of the partial tones of a compound are generally once, twice, three times, four times, the rate of the fundamental, and so on. In some cases, however, *e. g.* in bells and tuning-forks one or more of the partial tones may have vibration rates not represented in this series and discordant with the fundamental tone. In what follows, the regular series of partial tones is meant except where the contrary is specified.

On the physics and physiology of this matter and others treated in this and the preceding section cf. Tyndall, On Sound; Blaserna, Theory of Sound in its Relation to Music; Taylor, Sound and Music; Helmholtz, Sensations of Tone. The last is of course the great classic on all such matters; the next to the last is very simple and untechnical and perhaps the best for those approaching the subject for the first time.

82. Partial tones: Analysis with resonators. If resonators are at hand the demonstration of the partial tones will be easy. Sound on the instrument the tones to which the resonators are tuned, and notice that they resound strongly to these tones and less strongly or not at all to other tones adjacent in pitch. Then sound the tone to which the largest of the resonators is tuned, and try the rest of the resonators in succession. Notice that others also resound (at their own proper pitch), thus betraying the presence of the tones to which they are tuned, and thus the composite character of the tone under examination. Which resonators will "speak" will depend on the instrument used, reed instruments giving a long and perfect series, pianos and stretched wires a perfect series generally as far as the 9th or 10th partial, and stopped organ pipes a short series. If difficulty is found in knowing when the resonator is resounding, it will be found useful to apply it to the ear intermittently, alternating, for example, two seconds of application with two seconds of withdrawal.

83. Partial tones: Analysis by indirect means. *a.* By sympathetic vibration. This succeeds especially well with the piano. Press the *c* key and hold it down so as to leave its strings free to vibrate; then strike the *C* key forcibly and after a couple of seconds release it. The *c* strings will be found to be sounding. Repeat, trying *c-sharp* or *b* instead of *c*; they will be found not to respond. Repeat the experiment, substituting *g*, *c′ e′ g′*, and *c″*; all will be found to respond but in lessening degrees. Other keys between *C* and *c″* may be tried but will be found in *very* faint vibration, if at all. *b.* By beats. This will succeed best with a reed instrument, e. g., a parlor organ or the harmonical. By pressing the keys of the instrument only a little way down any of its tones may be sounded a little flatter than its true pitch and so in condition to beat with any other tone having that true pitch. Sound at this flattened pitch the over-tones of *C* in succession while *C* is sounding, and notice the slow beats that result. For verification sound other tones not over-tones of *C* and notice that the beats when present are much more rapid.

84. Partial tones: Direct analysis without special apparatus. The directions given here apply to the sonometer, but will be readily adaptable to any stringed instrument in which the strings can be exposed. It is easier to hear any partial tone in the compound, if the partial is first heard by itself and then immediately in combination with the rest. On strings this is easily done by sounding the partials as "harmonics." Pluck the string near one end (say about one-seventh of the length of the string from the end), and immediately touch it in the middle with the finger or a camel's-hair brush. The fundamental will cease to sound and its octave (the second partial) will be left sounding, as an "harmonic." With it sound also other even-numbered partials, but less strongly. Pluck as before and touch the string at one-third its length; the third partial will now sound out strongest, with the sixth, ninth, etc., more faintly. Thus by plucking the string and touching it respectively at one-half, one-third, one-fourth, one-fifth, one-sixth, one-seventh, one-eighth, one-ninth and one-tenth its length from the end, the series of tones corresponding to the 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th and 10th partials can be heard, each in large measure by itself. In getting the higher "harmonics" it will be found better to pluck nearer

the end than one-seventh, and in no case should the string be plucked at the point at which it is presently to be touched. (cf. Ex. 86 *b*.) To hear the partial tones when sounding in the compound, proceed as follows. Sound the required tone as an "harmonic," and then keeping the attention fixed on that tone, stop the string and pluck it again, this time letting it vibrate freely. The tone just heard as an "harmonic" will now be heard sounding with the rest as a partial. When the partial is thus made out, verify the analysis by touching the string again and letting the tone sound once more as an "harmonic." Try in this way for the partials up to the tenth; first for the 3d, 5th and 7th, afterward for the 6th, 4th and the 2d, which is the most difficult of all. It has been said that analysis is easier at night, (not alone on account of the greater stillness), when one ear is used, and that certain positions of the head favor certain partials.

85. Partial tones: Direct analysis without apparatus. Certain parts of a compound tone are sometimes so separated by their dissonance, intensity or pitch that they stand out with striking clearness. *a.* Strike a tuning fork on a hard surface and observe the high, ringing, dissonant partials. They fade out before the proper tone of the fork, and are heard best when the fork is not held near the ear. *b.* As the tone of a string is allowed to die away of itself, different partial tones successively come into prominence. Try with a low piano string, keeping the key pressed down while the sound fades, or on the sonometer. Something of the same kind, but less marked, happens in the dying away of a low tone on a reed instrument when the air is allowed to run low in the bellows. *c.* When a tone is sounded continuously for some time, for example, on a reed instrument with one of the keys clamped down, different partials come successively into prominence, either through varying fatigue or the wandering of attention.

Cf. Helmholtz, *op. cit.* pp. 36—65; Stumpf, *op. cit.* II, 231—243, see also the index under *Obertöne*.

86. Timbre. The peculiar differences in quality of tones, (distinct from pitch and intensity,) which are known as differences in timbre (tone color, clang tint, *Klangfarbe*), are due to differences in the number, pitch and intensity of the partial tones present. Compare in this respect the dull-sounding bottle tones or the tones of tuning forks held over resonance bottles and the more brilliant tones of a reed or stringed instrument; the first are nearly simple tones, while the second have strong and numerous over-tones. *a.* Notice the difference in quality between the tone given by a tuning fork held before the ear and that given by the same fork when its stem is pressed upon the table. In the second position the over-tones are relatively stronger. *b.* Notice the differences in quality in the tone of a string when it is plucked in the middle, at one-third its length and at about one-seventh. When plucked in the middle, many odd-numbered partials are present and the even-numbered partials are either absent or extremely faint, and the tone is hollow and nasal; when plucked at one-third, the third, sixth and ninth partials are wanting and the tone is hollow, but not so much so as before; when plucked at one-seventh all the partials up to the seventh are present (for their theoretical intensities cf. Helmholtz *op. cit.* p. 79). *c.* Try also plucking very near one end, plucking with the finger-nail and striking the string with a hard body, e. g., the back of a knife blade; all these bring out the higher and mutually discordant partials strongly and produce a brassy timbre. Cf. also Ex. 79 *b*.

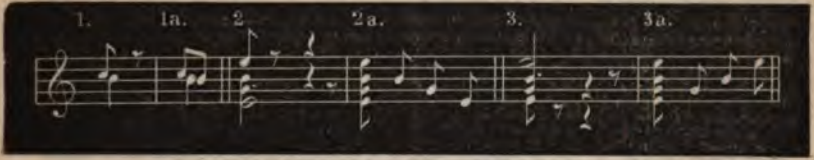
Cf. Helmholtz, *op. cit.*, pp. 65—119; Stumpf, *op. cit.*, II, 514—549.

87. In successive chords the whole mass of tone seems to move in the same direction as the part that changes most. Strike in succession the chords *e' g'-sharp b' e''*, *a a' c''-sharp e''* or *a c' e' c''*, *a c' f' c''*. If the attention is directed to the bass in the first example and to the alto

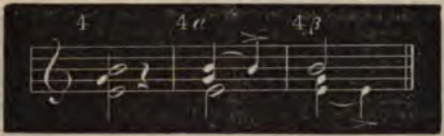
in the second the whole mass of tone will appear to descend in the first case and to ascend in the second. If the attention is kept on the soprano part the illusion will not appear, as also when the observer examines his sensations critically. Cf. also Ex. 77 *d* where beats of a partial tone are attributed to the whole compound tone.

Cf. Mach, Analyse der Empfindungen, 1886, 126–127; Stumpf, *op. cit.*, II, 393–395.

88. Simultaneous tones interfere somewhat with one another in intensity.



a. Play the groups of notes numbered 1, 2 and 3 and observe the slight increase in the apparent intensity of the remaining tones as one after another drops out, making 1 sound like 1a, 2 like 2a, and so on. On the piano it will be well to play the notes an octave or two deeper than they are written.



b. Play the notes marked 4 and notice that the increase of loudness seems to affect the note (highest or lowest) that receives particular attention, making the effect in one case like 4α, in the other like 4β.

Cf. Mach, *op. cit.* 126; Stumpf, *op. cit.*, II, 418–423.

89. Consonant and dissonant intervals. *a.* The consonant intervals within the octave are the unison, octave, fifth, fourth, major sixth, major third, minor third, minor sixth. They will be found to decrease in smoothness about in the order given. Try them beginning with the octave and at *c*, as follows: *c c′*, *c g*, *c f*, *c a*, *c e*, *c e-flat*, *c a-flat*. Try the last four intervals also in the octave of c'' or c''' and notice that they are less rough than when taken in the octave of *c*. Any other intervals within the octave are dissonant. Try *c c-sharp*, *cd*, *cb*, *cb-flat*, *c f-sharp*. The roughness is due to beating partial tones and in general is greater when these stand low in the series and are loud, and when they lie within a half-tone of each other. Work out for the tones of several of the intervals the series of partial tones up to the eighth. In general the extension of intervals into the second octave (taking the higher tone an octave higher or the lower tone an octave lower) does not change the fact of consonance or dissonance, though it may change the relative roughness. *b.* Those fitted by musical training to pronounce upon questions of consonance and dissonance hold that dissonance can be perceived between simple tones under conditions that exclude beats, and that consonance is not simply the smooth flowing of tones undisturbed by beats. The test is easy to make—simply to hold tuning forks making the intervals to be tested one before each ear, and if there are beats to carry the forks far enough away in each direction to make the beats inaudible—but only those of musical ear can pronounce upon the result.

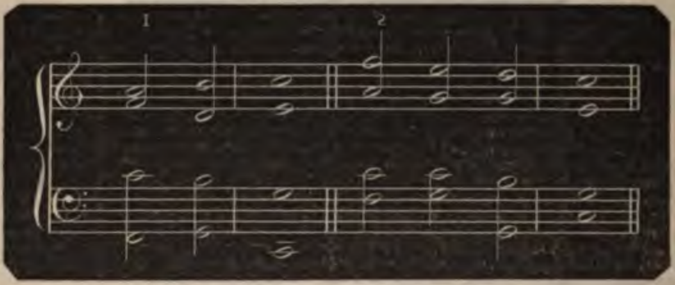
Cf. on *a*, Helmholtz, *op. cit.*, pp. 179–197. Stumpf, *op. cit.* II, 470, 460. Wundt, Physiologische Psychologie, I, 439, II, 47 ff.; Mach, *op. cit.*, 129–130.

90. Consonant and dissonant chords. In order to form a consonant chord all the intervals between the tones used must also be consonant. The only chords of three tones which fulfil this condition within the octave are represented by the following: Major *c e g*, *c f a*, *c e-flat a-flat*, minor *c e-flat g*, *c f a-flat*, *c e a*. Try these and for comparison any other chord of three tones having *c* for its lowest tone.

Cf. Helmholtz, *op. cit.* p. 211 ff.; Wundt, *op. cit.* II, 61, 67 ff.

91. Major and minor chords. Compare the chords c'' e'' g'' and c'' e''-*flat* g''. This unmistakable difference in effect depends in part at least on the fact that in the major chord the difference tones of the first order are lower octaves of c'' itself, while in the minor chord one difference tone is not such at all and if taken in the same octave with the chord would be highly dissonant. For the major chord, when taken in the octave of c'', the difference tones are *c* and c'', for the minor chord *c*, *e-flat*, *A-flat*. Try on a reed instrument the difference tones generated by c'' e'', e'' g'', c'' e''-*flat*, e''-*flat* g'', first separately; and then while c'' and g'' are kept sounding, strike e'' and e''-*flat* alternately.

92. Cadences. Modern music requires the prominence of the key note or tonic and of the chord in which it holds the chief place at the beginning of a piece of music and at the end. The feeling of the appropriateness of this close and especially of the succession of chords in the following cadences can hardly fail to appeal even to the unmusical.



Cf. Helmholtz, *op. cit.*, 293.

BINAURAL AUDITION AND THE LOCATION OF SOUNDS.

Apparatus. In addition to apparatus already used, a pair of unison tuning-forks on resonance cases will be needed in Ex. 96 *d*, (and in several of the other experiments such large forks, unscrewed from their cases, are almost indispensable, because the tones of ordinary small forks are too faint and last too short a time), also a mechanical telegraphic "snapper-sounder," a yard-stick and a retort stand. The "snapper sounder," common as a toy a few years ago, can be bought of E. S. Greeley & Co., 5 & 7 Dey St., New York, at from 30 to 75 cents.

93. Unison tones heard with the two ears. *a.* Strike a pair of unison forks that will sound equally loud and vibrate an equal length of time, and hold one before each ear, three or four inches away; a single tone of rather indefinite location will be heard. As the forks are brought nearer, their tone seems to draw by degrees toward the median plane; and when they are very loud and near, the tone may seem to be in the head. Return the forks to their first position and then move one a little nearer or a little farther away, and notice that the sound moves to the side of the nearer fork. When the difference in distance has become considerable that fork alone will be heard. *b.* Bring the forks again into the positions last mentioned—one near and one far, (or better, place one fork on a rubber tube one end of which has been inserted in

the opening of the ear and hold the other fork before the other ear), and then with the free or more distant fork make slow rythmical motions toward and away from the ear, or rotate the fork slowly about its long axis, attending meantime to the fork on the other side. Alternate variations in the intensity of the tone of this fork corresponding to the approach and recession of the other and apparently unheard fork can be heard. *c.* Repeat *b* and notice that when the changes in intensity are considerable there is a simultaneous shifting of the place of the tone, toward the median plane when the tone grows stronger, and away when it grows fainter. These changes of place are, however, less marked than changes in intensity and those accompanying slight changes in intensity generally escape observation.

Cf. Schaefer, Zur interaurealen Lokalisation diotischer Wahrnehmungen, Zeitschrift für Psychologie, I, 1890, 300-309; also Silvanus P. Thompson, On Binaural Audition, Phil. Mag. Series 5, IV (July-Dec., 1877) 274-276; VI (July-Dec., 1878) 383-391, XII (July-Dec., 1881) 351-355; On the Function of the two Ears in the Perception of Space, XIII (Jan.-June, 1882) 406-416; and the references given by these two authors.

94. Beats heard with the two ears. *a.* Operate as in Ex. 93 *a*, with forks beating three or four times a second. *b.* Try with a pair of very slow beating forks (once in two or three seconds). Notice a shifting of the sound from ear to ear corresponding to the rate of beating. *c.* Try again with a pair of rapid beating forks (twenty or thirty a second) and notice that the beats are heard in both ears.

Schaefer, *op. cit.* also Ueber die Wahrnehmung und Localisation von Schwebungen und Differenztönen, Zeit. f. Psy. I, 1890, 81—98.

95. Difference of location helps in the analysis of simultaneous tones. Compare the ease with which the tones of a pair of octave forks are distinguished when the forks are held on opposite sides of the head with the difficulty of analysis in Ex. 79*b*.

Cf. Stumpf, *op. cit.* II, 336, 363.

96. Judgments of the direction of sounds. These depend in general on the relative intensity of the sounds reaching the two ears, but there is pretty good reason to believe that other things co-operate and that tolerably correct judgments, both as to distance and direction, can sometimes be made from the sensations of one ear. *a.* Let the subject be seated with closed eyes. Snap the telegraph snapper at different points in space a foot or two distant from his head, being very careful not to betray its position in any way, and require him to indicate the direction of the sound. Try points both in and out of the median plane. Observe that the subject seldom or never confuses right and left but often makes gross errors in other directions. Constant tendencies to certain locations are by no means uncommon. *b.* Have the subject hold his hands against the sides of his head like another pair of ears, hollow backward, and try the effect upon his judgment of the direction of the snapper. *c.* Find approximately how far the snapper must be moved vertically from the following points in order to make a just observable change in location: on a level with the ears in the median plane two feet in front; opposite one ear, same distance; in the median plane behind the head, same distance. Find the just observable horizontal displacements at the same points. A convenient way of measuring these distances is to clamp a yard-stick to a retort-stand, bring it into the line along which measurements are to be made and hold the snapper over the divisions of the stick. Snap once at the point of departure, then at a point a little way distant in the direction to be studied; again at the first point, so that the subject may keep it in mind, and then at a point a little more distant, and so on till a point is finally found which the subject recognizes as just observably different. Repeat, alternating snaps at the point of departure with those at a greater distance than that just found, decreasing the latter till a point is found where the directions can be no longer distinguished. Make a number of tests each way and take their average.

d. Continuous simple tones are very difficult to locate. Place a tuning-fork on its resonance case at some distance in front of the subject (seated with closed eyes), another at an equal distance behind him. With the help of an assistant strike both forks and after a little have one of them stopped and the mouth of its resonance box covered. Require the subject to say which has been stopped. His errors will be very frequent. Compare with this his ability to distinguish whether a speaker is before or behind him.

Cf. on *a* Preyer, Die Wahrnehmung der Schallrichtung mittelst der Bogengänge, Pflüger's Archiv, 1887; also v. Kries, Ueber das Erkennen der Schallrichtung, Zeit. f. Psy. I, 1890, 235-251. On *c* cf. Münsterberg, Raumsinn des Ohres, Beiträge zur experimentellen Psychologie, Heft. II, 1889. Rayleigh, Nature, XIV, 1876, 32.

97. Intercranial location of sounds. *a.* Sounds originating outside the head are not located in the head when heard with one ear. Hold a loud sounding tuning fork near the ear or place it on a rubber tube, one end of which is inserted in the opening of the ear, and notice that the sound when strong may be located in the ear, but does not penetrate further. Insert the other end of the tube in the opening of the other ear and repeat. The tone if loud will appear to come from the inside of the head. Removing and replacing the fork several times will help to give definiteness to the location. *b.* Repeat the experiment, but use a fork sounding as faintly as possible (*e. g.* set in vibration by blowing smartly against it), and notice that the location when a single ear receives the sound is not so clearly in the ear, and, when both receive it, not so clearly in the head, perhaps even outside of it. Cf. also Ex. 98 *b.* These experiments may also be made with beating tones instead of a single one.

Cf. Schaefer, *op. cit.* under 94.

98. Location of the tones of tuning-forks pressed against the head. *a.* Strike a large and loud sounding tuning-fork and press its stem against the vertex. The tone will seem to come from the interior of the head chiefly from the back. While the fork is in the same position close one of the ears, not pressing it too tight; the sound will immediately seem to concentrate in the closed ear. Have an assistant manage the fork and close the ears alternately. Something of the same kind happens when a deep note is sung; close first one ear and then both and notice the passage of the tone from the throat to the ear and finally to the middle of the head. *b.* Have an assistant manage the fork and close both ears. Notice that when fork is pressed on so as to make the tone loud the intercranial location is exact, but when the pressure is relaxed and the tone is faint the location tends to be extracranial. *c.* Try setting the fork on other places than the vertex. Notice that in the occipital and parietal regions the sound appears in the opposite ear. *d.* Take a long pencil in the teeth like a bit and rest the stem of a tuning-fork vertically on it near one end and close the ear on the other side; the sound will seem to be located in the closed ear. Then gradually tilt the fork backward toward a horizontal position, keeping it in contact with the pencil till its tip is opposite the open ear. The tone will change its place from the closed to the open ear.

On *a* and *b* cf. Schaefer, *op. cit.* under 94; on *c* cf. Thompson, second article referred to under 94.

PSYCHIATRY.

Psychoses Following Acute Surgical and Mental Affections and in Multiple Neuritis.

By William Noyes, M. D.

Introductory Note. Comparatively little attention has been given to the mental condition in this class of affections, and the importance of the subject suggests the advantage of presenting at some little length the more recent opinions of different writers. It is the description of the mental state of patients suffering from the disorders to which attention is especially directed, and there can be but little doubt that it would be a great gain to psychiatric nomenclature if Prof. Wood's title of *Confusional Insanity* could be generally adopted. Under such circumstances, "Confusional Insanity Following Typhoid," or "Confusional Insanity after Hysterectomy," would designate a distinct clinical entity and would occupy as proper a place in statistics and classifications as "Dementia Secondary to Mania." Prof. Wood's description of the mental state is a peculiarly graphic and vigorous one, and merits a permanent place in literature. It will be noted that Wood and Korsakoff takes opposite views as to the etiology of affection, for Korsakoff states that his *cerebropathy* occurs also after exhausting diseases, so that he and Wood are evidently describing the same affection. The occurrence of the affection in multiple neuritis, a distinctly toxic disease, goes far towards bearing out Korsakoff's views, and yet Wood's arguments against the toxic origin after acute surgical affections certainly have much weight. While, then, we may not at present look on the etiology as settled, there will be much gained if a distinct clinical picture can be agreed upon.

Insanity after Acute Surgical or Medical Affections. H. C. Wood, M. D. University Medical Magazine, December, 1889.

The author deprecates a tendency in writers on Insanity to recognize as distinct diseases several varieties of mental disorder, which he thinks should be viewed simply as symptom groups. The evidences of mental disorder may vary when the brain lesion is apparently the same, so that an individual case may appear to belong now to this and now that special insanity. Congestion, even active congestion, may go hand in hand with exhaustion, and failing nutrition even predisposes to local affluxes of blood, by producing weakness of the vessels, and according as now this and now that region of the brain is invaded by these local changes in circulation, so will the symptoms shift from day to day. The fact that two cases for a time prevent similar manifestations is no proof that they are essentially the same in their underlying cerebral condition, and the circumstance that they offer diverse symptoms is no proof that they are essentially unlike.

Wood believes that although insanity following acute disease varies greatly in its symptomatology, that in almost all the cases there is one common fundamental brain condition and that this fundamental brain condition bears no specific relation to the disease which has produced it, but may be the outcome of an altered nutrition produced by an exanthematous disease, like typhoid fever, or by a diathetic disorder, like rheumatism, by an accidental traumatism, or by a surgical operation. There are etiological and symptomatical reasons for believing that these insanities after acute disease are identical in their nature.

Etiological. If we believe that the insanity has a specific relation to the poison of the disease which it has followed, we must consider that there are at least a half dozen specific insanities connected with acute diseases, a very improbable supposition. The symptoms develop at a time when the specific action of the poison upon the nervous system has exhausted itself, namely, during convalescence. The insanities develop after diseases or affections in which there is no known specific poison, such as childbirth, traumatism. surgical injuries, fevers, etc., which are followed by insane outbreaks, have one influence in common, i. e., they all tend to exhaust or impair the nutrition of the nerve centers, and it is known that impairment of the nutrition of the centers by lack of food combined with anxiety is capable of causing symptoms similar to those which are present in insanities developed after disease.

Symptomatical. Though the cases vary much in their details, the general scope of the symptoms and the general course of the disorder are identical. There is always mental confusion, a mixture of excitement and mental power; and the cases nearly always end in complete recovery from organic disease.

Wood compares this condition to Krafft-Ebing's "Stupor; or Primary Curable Dementia," which is a condition of cerebral exhaustion in which there is almost complete paralysis of the mental functions with loss of nerve-tone in every portion of the body, so that the patient remains in a condition of more or less profound stupor or stupidity, with shifting or kaleidoscopic anomolies of motor and vaso-motor innervation, and at times also gives evidences of delirium, or of hallucinations. This primary dementia may be produced by starvation, by profound emotional shock, by diseases of the basal blood vessels, and it is asserted even by injuries to the head (?). If old age, syphilis or gout has produced excessive degeneration in the cerebral blood vessels, or if an emotional shock has been so severe as to permanently alter the nutrition of the cerebral nerve-cells, this so-called "Curable Dementia" may be an incurable affection.

Krafft-Ebing also recognizes under the name of *Wahnsinn* an affection which has been known to English and American writers as "Delusional Stupor," "Mania Hallucinatoria," "Confusional Insanity," etc., in which there is active delirium associated with an extraordinary abundance of hallucinations present in every sense region, and with a great weakness of the whole nervous system, as shown by pronounced loss of mental power almost amounting, it may be, to imbecility, by a tendency to stupor, by lack of muscular power, and by failure of nerve-tone in every portion of the organism. Krafft-Ebing considers that the two affections, *Wahnsinn* and Primary Curable Dementia clinically grade into one another, and that the underlying brain affection is similar in each affection. Wood believes that these two so-called diseases are merely diverse manifestations of one and the same pathological condition, and that they should be considered as one disease, and he suggests that the name *Confusional Insanity* be given to the condition because it is already familiar to many, has no pathological import, and expresses a symptom which is not only common to all forms of the disease, but is a necessary outcome of the pathological state.

In various chronic diseases attended with great bodily and mental exhaustion, the brain tissue gradually passes into a condition of perverted and exhausted nutrition similar to that of Confusional Insanity. In long drawn out cases of consumption there is often a gradual impairment of the intellect, associated with a super-activity of the imagination, and especially during the night the patient becomes delirious. Almost every history of shipwreck, followed by long exposure and starvation, affords examples of failing mental power, accompanied by increasing activity of the imagination, until desire and thought-pictures give rise to hallucinations, which are at first recognized by the sufferer to be false, but finally lure him to leap overboard.

The underlying nerve condition in each of these cases is one of a peculiar exhaustion, and it would appear tnat almost any form of acute exhausting disease may be followed by a similar mental state. Wood has reported a case of so-called acute gouty insanity which he considers represented primary dementia, the stupor form of confusional insanity, and to his mind gouty and rheumatic insanity are probably almost always representatives of this disease.

Confusional insanity follows typhoid fever not very infrequently, and probably constitutes the bulk of the cases commonly named puerperal mania. To it, also, belongs the so-called surgical insanity. Within one year Wood saw it develop after ovariotomy, perineorrhaphy, and after the removal of the breast for cancer. It may also be due to emotional strain, especially when this is sudden or accompanied by exhausting circumstances.

In the mildest cases of mental disorders after acute exhausting disease the only symptoms may be enfeeblement of the general mental powers. In many cases the original mental constitution is recovered very slowly, being possibly slower in mild than in severe cases. This mental enfeeblement may be associated with depression of spirits, but this is not so intense or so overpowering as in melancholia, and the emotional disturbance is not the dominant element in the case. When confusional insanity is fully developed there is almost invariably a general lack of nerve-tone, as shown by a feeble circulation and coldness of the extremeties, by general muscular relaxation, and by failure of the digestive power. The temperature varies in different cases. It may be normal, but in severe cases there is usually either an habitually low temperature or a marked tendency to paroxysms of sub-normal temperature. On the other hand there may be a very distinct febrile reaction, especially seen in puerperal cases. The temperatnre curve is often remarkably irregular. The mental symptoms may seem to be contradictory, since many of them are those which are commonly believed to be the outcome of paralysis of cerebral functions, and others are such as are sometimes thought to be evidence of excited, though perverted cerebral activity. In the first group belongs that depression of consciousness, which in its mildest forms may be shown only by a peculiar quietude and by apathy, but which in varying degrees of greater severity manifests itself by stupor, ever growing, as the disease becomes more severe in intensity, until it deepens into a complete, persistent loss of consciousness. Another outcome of cerebral weakness is the peculiar mental confusion which is the most characteristic manifestation of the disease. It may reveal itself chiefly in the inability of the patient to talk coherently and persistently, words dropping out of the sentence or being uttered imperfectly, because the mind is unable to get the right word, ideas changing in the middle of a sentence, because the power of confining the attention to one consecutive line of thought is lost, so that the attempt at conversation on the part of the patient results in a jumble of half sentences, clauses and words, hopelessly intermixed with one another. Even, however, in mild cases of disease, the mental confusion usually manifests itself not merely in the inability of the patient to hold a connected conversation, but in his want of power to appreciate persons and things about him. In the most extreme instances no objects or faces are recognized, and even in the very mild forms of the disorder the patient may recognize some of his friends, yet be unable to place himself, insisting that he is away from home, and pathetically begging to be taken to his own house. The confusion of the patient is not altogether the outcome of pure mental weakness, but is usually in part due to the extrordinarily numerous and vivid hallucinations which affect all the senses, and compete for recognition, by the consciousness, with impulses which really originate in external objects.

The delirium is commonly mild and lacking in aggressiveness, but it may take on a vary active form, or the patient may be habitually quiet but subject to paroxysms of fury resembling those of acute mania. More commonly, however, underlying even the aggressiveness and violence, there is a foundation of fear which often resembles that of delirium tremens, and when with this condition of fear there is associated distinct tremulousness, the likeness to delirium tremens is very pronounced; indeed, Wood believes that delirium tremens should be considered a form or variety of confusional insanity.

Very rarely ought there to be any trouble in recognizing the true nature of confusional insanity. The history of the attack, the knowledge that the outbreak was preceded by an exhausting disease, traumatism or emotion, the failure of bodily nutrition and of general nerve force, the lack of dominant emotional excitement, the stupor, the peculiar mental confusion, the kaleidoscopic character of the hallucinations, make diagnosis easy. The propiosis is favorable. Krafft-Ebing gets 70% of recoveries, and in Wood's cases even when the mental confusion has amounted to complete and absolute imbecility; complete recovery has almost invariably occurred, provided there have been no preëxisting organic bodily lesions, such as unsound kidneys, or degenerated arteries. Death may, however, occur in complicated cases. If the mental recovery be not complete, the result is lack of mental power, but never a so-called reasoning insanity, never a state resembling that of paranoia. Wood cites five cases illustrating his conception of confusional insanity: I., after childbirth; II., after removal of the breast for cancer; III., after perineorraphy; IV., after typhoid; V., after loss of sleep from nursing, combined with anxiety. All the patients recovered.

Cases of Post-Febrile Insanity. WILLIAM OSLER, M. D. John Hopkins' Hospital Reports, 1890, II, 46.

This article is written to give illustrative cases of Wood's Confusional Insanity, where there is one fundamental brain condition, viz:—impaired nutrition with consequent exhaustion of the nerve centres. Osler refers to the articles by Shepard (Am. J. Med. Sciences, Dec., 1888), and T. Gaillard Thomas (Medical News, 1889), and reports five cases:

I. Pneumonia. Slow convalescence with development of hallucinations and delusions.
II. Typhoid fever; severe attack with much delirium. Mania during convalescence. Gradual recovery after four months.
III. Typhoid fever of moderate severity. Development of delusions during convalescence. Recovery after six weeks.
IV. Typhoid fever, mild attack. Gradual development of delusions. Slow, halting speech. Recovery.
V. Typhoid fever, severe attack. During convalescence development of delusions. Persistence of mental symptoms for ten weeks. Recovery.

Prognosis usually good. Of the seven cases seen by Osler five after typhoid and two after pneumonia, six recovered and the seventh seemed likely to recover. Patients should therefore be cared for at home if possible. Seclusion, incessant watchfulness, absolute rest in bed, with massage and careful feeding are indicated. In the cases where the temperature is mentioned this had fallen to normal before the mental symptoms came on.

Osler does not attempt to add to Wood's description of the mental state of these patients.

Acute Confusional Insanity. CONALLY NORMAN. Dublin Journal of Medical Science, 1890, I, 506.

Norman claims that this form of insanity is not recognized in England. He agrees with Salgó that acute confusion is the most common of all forms of insanity, although Salgó's definition is too wide according to Norman. It would come between the acute mania and acute primary dementia of Pinel. It is a condition of mental disturbance of comparatively rapid onset, characterized by dream-like engagement of consciousness and a tendency to abundant hallucinations of one or more senses. As the confusion or the hallucinations predominate the case resembles acute dementia or mania (melancholia.) Predominance of confusion corresponds to the delusional stupor of Newington; predominance of hallucinations corresponds to Mendel's hallucinatory mania. Norman finds hallucinations less frequent than other authors, and quotes Meynert as giving up the term acute hallucinatory insanity (Wahnsinn) for confusion. It is acute in onset; in form, acute or peracute, more frequently sub-acute. True chronicity hardly exists, except in uncured cases lapsing into secondary mania. Usually begins with hallucinations. Recovered patients speak of a dreamy obscuration of the mind; this frequently escapes observation. Consciousness profoundly affected; unoriented; confused as to time; varying and disconnected delusions flit through the mind, which are accepted as we accept dreams. Hallucinations may be pleasant or the reverse, following the emotional state of the patient. Emotional state generally indifferent, without pleasure or pain. Emotional condition variable as distinguished from mania or melancholia, sometimes gay, sad, anxious, angry, tender, or all these things together or in most rapid succession. Emotional disturbance is a reactive one, arising from the nature of the hallucinations. Acts as well as feelings are dictated by hallucinations. Episodic reactive states of emotional excitement or motor restlessness are apt to be followed by periods of increased confusion, deepening into stupor, or stuporous conditions intervene directly. Agrees with Krafft-Ebing that acute confusional insanity is essentially a condition of brain exhaustion, and probably due to brain anaemia or malnutrition of cortex. Patient is usually feeble and anaemic, or has recently suffered from some exhausting disease. This is more often than any other the form of psychical disorder associated with diseases not primarily affecting the nervous centres. Puerperal insanity is generally of this form, and the same of the insanity of rheumatism, and the delirium of fevers occasionally passes directly into acute confusion. Prolonged lactation, chronic suppurative affections, diseases of the stomach and of the lungs, especially phthisis, have a strong predisposing, if not exciting influence. Krafft-Ebing describes it as arising in prisoners. Norman has found it associated with nostolgia. Also occurs in cases of sexual excess or irregularity, generally with hallucinations. One case followed mental shock; and it is to be noted that the most common form that insanity takes when it follows sudden shock is the kindred one of acute dementia. Norman considers the well marked form of insanity following drink as acute confusional insanity, which is usually described as something between *delirium tremens* and acute mania. There is loss of orientation, dream-like impairment of consciousness, and numerous hallucinations. Dreamy confusion is more common in women. James Ross has described a confusion characteristic of dementia accompanying alcoholic neuritis. Wigglesworth confirmed Ross's observation, and in 1887 Korsakoff described in connection with alcoholic neuritis a "form of confusion with extremely characteristic mistakes in relation to space, time and situation." The onset is often acute. The insanity which comes "out of sleep" is always of this type. This brings it into line with that state occasionally present in the sane and especially in those of neurotic tendency and in epileptics, called by Germans *Schlafkrankheit*. Duration may be short, lasting only a few days or a few hours in abortive cases (as in some cases of menstrual disturbance), as Krafft-Ebing points out. Krafft-Ebing puts his recoveries at 70%. Cases which are about to recover occasionally pass into a state resembling acute mania, first observed by Meynert, who thought that the functional hyperaemia accompanying the maniacal attack brought on a tendency to cure by increasing the circulation of blood through the exhausted brain. A slight degree of stupor more frequently precedes recovery, as in convalescence from acute mania. A mixture of maniacal and stuporous conditions is less favorable, or a tendency towards histrionic and pathetic displays, or the occurrence of pseudo-tetanic or pseudo-cataleptic states. Latter symptoms approximate Catatonia, which indeed is probably to be regarded as a variety of the general affection under consideration. As in all acute insanities death from exhaustion may occur in the early stage, and in debilitated sufferers there is a tendency to succumb to intercurrent affections. The diagnosis lies between acute mania, acute melancholia, acute dementia, and certain forms of paranoia. From mania it is distinguished by absence of exaltation and of increased rapidity of thought. (Norman, with *Salgó*, would exclude from mania any case with hallucinations). True emotional depression as a primary symptom is absent in acute confusion, distinguishing it from melancholia. It is intimately associated with acute dementia, and it is not always possible to say which form we are dealing with, though the presence of hallucinations and absence of complete stupor in a typical case of complete confusion sufficiently denote the ailment. Distinguished from paranoia by want of systematization of delusions, by existence of confusion, and by sudden mode of origin.

Norman repeats nine cases as follows:

I. Acute confusion, associated with alcoholic excess. Neuritic pains; recovery.
II. Acute hallucinatory confusion, associated with alcoholic excess; epileptiform seizures; recovery. "An extremely typical case of alcoholism in a woman."
III. Acute hallucinatory confusion associated with alcoholic excess.
IV. Confusion in the special form described by Ross and Wigglesworth occurring in a toper. Passage into secondary dementia.
V. Acute hallucinatory confusion resembling paranoia, associated with alcoholic excess. Recovery.
VI. Acute hallucinatory confusion simulating paranoia, following rheumatism and perhaps associated with nostalgia. Recovery.
VII. Hallucinatory confusion associated with phthisis.
VIII. Acute hallucinatory confusion dependent perhaps upon nostalgia. Passage into dementia.
IX. Acute hallucinatory confusion beginning in a dream. Apparent cause, sexual irregularity.

Norman cites Korsakoff's articles and considers that they are both describing the same form of mental disturbance.

Folie post-opératoire. PROF. MAIRET. Le Bulletin Medical, 1889; Aug. 28 and Sept. 1.

Prof. Mairet studies the mode of evolution of insanity following operations rather than the form of the insanity itself. He adds one case to literature, that of a woman of 42 who became insane three days after a laparotomy. Patient was intelligent and vivacious, but without hereditary or degenerative nervous taint. At 22, after childbirth, she suffered from attacks of hysteria with syncope, without absolute loss of consciousness, but with delusional troubles following, and hallucinations of sight and hearing; attacks sometimes lasted minutes, at other times hours; troubles appeared with menses. Intellect unimpaired, and she retained the management of her household. At 39 abdominal trouble appeared, necessitating laparotomy three years later. Three days after the operation began to laugh without motive and to have hallucinations of hearing. Delusions increased and patient admitted to asylum three months and a half after operation. Torpor and intellectual wandering were most noticeable. Marked failure in nutrition. Refused to eat. Died of exhaustion 35 days after admission.

Mairet raises the question whether the sudden appearance of the mental trouble after the operation was simply a coincidence, or if the operation had a distinct etiological relation, and decides in favor of the etiological relationship. He finds in literature 24 cases where insanity followed operation, but admits that the list may not be complete. In analyzing these cases he finds that the rôle of the operation may be a variable one, being at times only an occasional cause, as in an operation in a case of alcoholism, while in other cases the operation plays a considerable part. There was considerable predisposition in his own case, while in a case reported by Herm-Lossen and Fuerstner the etiological importance of the operation was still greater, and the predisposition much less, there being no nervous predisposition except a chorea at the age of 14. In reviewing the cases Mairet reaches the conclusion that a certain amount of predisposition is always necessary, and that a surgical operation by itself is not capable of producing insanity. While the most different operations may be followed by insanity, are all operations susceptible in the same degree of producing it? It is the grave operations, especially those on abdominal viscera, that cause insanity without there being a strong predisposition. Werth reports the following results: Two cases of insanity in 32 hysterectomies, or 6%; two in 36 castrations, or 5.55%; and only two in 160 ovariotomies. Regarding the manner in which surgical operations produce insanity Mairet holds that in a surgical operation of considerable importance the surgical traumatism and its sequelæ are not the sole elements susceptible of working on the brain and thus developing insanity. For a certain time before the operation the patient is preoccupied; he dreads the operation, and his mind is in a state of tension that particularly favors the development of insanity. The anaesthetics, too, have a particularly strong action on the nervous system, and especially upon the brain. After the operation the surgeon uses in the dressings substances such as iodoform, which are in themselves capable of producing mental troubles; and finally, after the operation the patient must be excited by stimulants, particularly by alcoholic drinks. Mairet is convinced that insanity after operations is the result of these different causes, or at least of several of them. Although he attaches but slight importance to iodoform, he attaches much to the etiological influence of anaesthetics.

In cases where the predisposition is feeble it is necessary to go to the operation itself, as such, to explain the development of the insanity, but the published facts are too few to assist in ascertaining how the operation works. It is not so much the operation itself, properly so called, as it is that the traumatism is succeeded by a more or less extended and severe wound. In the cases reported the operation itself and its sequalæ have been absolutely regular. It is on the side of the nutrition that it is necessary to look for the reason of the action of the traumatism. In Mariet's case the troubles in nutrition appeared directly after the operation, but the observations are too few to say that this is always the case. This was markedly the case in Shepard's two cases. However this may be, the troubles of nutrition when they exist put the nervous system in a state of morbid receptivity which allows the passing delirium which the anaesthetics and the other causes may produce, to pass into a chronic state and to favor the production of true insanity.

As regards the time of onset of the mental disturbance, this may come on immediately after the operation, but generally it is not until several days, usually on the 3d or 5th day, or at least within the first week that the patient is perceived to be strange and to have lost his mental equilibrium. Sometimes, however, the mental troubles do not appear until later. Werth reports cases in the 2d, 3d and 5th weeks. Usually the development takes place progressively—a modification of character and illusions appearing first, then agitation is added, and finally after a longer or shorter time, days or weeks, the insanity is definitely established. More rarely the insanity sets in suddenly without prodromes. Meredith reports a case where an acute melancholia appeared suddenly at the beginning of the 4th week.

Post-operative insanity has different forms, and here must be distinguished the cases in which the operation acts only as a provoking cause and those in which its pathogenic influence is considerable. When the operation plays only the rôle of an occasional cause the form which the insanity takes is dependent not on the traumatism but on the anterior state, which may be of a very variable nature. In one case it may be a very powerful predisposition or an intoxication, or in another case it may be a typhoid fever which modifies the central nervous system. When the pathogenic influence is most powerful the forms which are generally found are mania and melancholia, but the observations are too few to say if the mania or melancholia have a special physiognomy. [Wood's remarks on this subject seem to be of much greater value.]

As regards prognosis this depends largely on whether the operation plays the part of a primary or an occasional cause, being more grave where the antecedent predisposition is more marked, and where the nutrition is poor, and here leading to incurable insanity or to death.

In summing up the whole subject Mairet concludes:

1°. It is among the predisposed individuals, predisposed either by heredity or any other cause (alcoholism, infectious diseases, etc.), that surgical operations give rise to insanity.

2°. Among the constituent elements of an operation that may act on the brain the two most important from the point of view of the development of insanity are anaesthetics and surgical traumatism with their sequelæ, chief among which are the troubles of nutrition.

3°. When the predisposition is considerable the anaesthetics may of themselves alone set this into activity and cause the appearance of insanity, so that the less important operations, acting as surgical traumatism, may give rise to insanity.

These points should govern the conduct of the physician in interfering surgically in predisposed individuals. Among these individuals one ought not to undertake an operation of any consequence except when there is a vital necessity, and when it has once been decided upon, anaesthetics, at least general anaesthetics, should be omitted if possible. [It need scarcely be pointed out that Prof. Mairet goes to extremes that few or any would care to follow in ascribing such overwhelming importance to anaesthetics. Were anaesthetics withheld to the extent he advocates, from the remote possibility of mental disturbance, much needless suffering could not fail to result.]

Insanity following Surgical Operations. LAWSON TAIT. British Medical Journal, Aug. 31, 1889. (Abstracted in Dublin Journal of Medical Science, 1890, I, 250.)

This is a criticism of the book of Dr. E. Denis on this subject. Tait says that he has performed between 7,000 and 8,000 operations, requiring the use of anaesthetics, and has had anaesthetics administered in cases not involving traumatism in 3,000 more instances, and he knows of only seven cases of sequent—not necessarily consequent—insanity. There may have been other cases, and he will say 14 cases to cover the margin of error. His own practice therefore does not yield a proportion of cases of insanity following operations larger than the general proportion of insanity in the female adult population, and including the cases of anaesthesia is probably considerably smaller. Dr. Denis gets an average of 2.5 cases of alienation in 100 operations. But if this had been the case all engaged in active operating practice would have felt the fact long ago. Tait is struck by the occurrence of insanity after operations as being like the occurrence of tetanus, something to be met with occasionally, but not a matter to calculate on. He continues: "If I saw an insanity rate of 2.5% in my operations it would be more striking than any death rate in anything except my hysterectomies, and in that class I have never seen insanity follow a single instance; and Dr. Bantock's experience amounts to practically the same result, for his exception cannot really be called one of insanity following an operation. As a per contra I can point to 13 cases where operations have cured insanity."

Ueber Psychosen nach Augen-Operationen. Von FRANKL-HOCHWART. Jahrbuch f. Psych., 1889-90—IX, pp. 152-182.

The author reports 31 cases of psychoses developing after eye-operations. Divided into four groups, as follows:

1. Hallucinatory Confusional Insanity. (a) in young, (b) in old individuals.
2. Simple Confusional Insanity in old people.
3. Psychoses in chronic alcoholism.
4. Cases of Confusional Insanity in very marasmatic individuals, with other intercurrent somatic diseases with fatal termination.

The first group comprised 15 cases; lens extraction in almost all, began six times in the first 24 hours, twice after two, once each after three and four days, twice after several days, once after nine days, once after ten to twelve days, once after three weeks. There was a Protean-like change of phenomena in the different individuals; there was wild, unmanageable agitation, ideas of grandeur and insignificance, ideas of suicide, ceaseless cryings, praying, lamenting, and then laughing, dancing and singing, with passionate emotional displays. These more sharply defined prodromal symptoms belong more to youth; in older people there is unrest, confusion and tendency to aggression, and also terrible visual and auditory hallucinations. Disease is usually fully developed when the patient is transferred to the asylum.

Regarding the course of these psychoses it can only be said that this is a very varying one. Some last a few days, and from that up to weeks, or to one, two or five months. One patient formed a complete delusional system of persecution after he had been three months in the asylum.

In the group of alcoholics there were seven patients, six of whom had cataract operations. Course offers little that is noteworthy; begins earlier than in non-alcoholics. Shows itself in restlessness and excitement. Course marked by unrest, hallucinations, conditions of anxiety, ideas of persecution, confusion, delusions. Course similar to delirium tremens. Lasts from 6 to 12 days to 4 weeks. Some dementia in one case.

In the first group (hallucinatory confusional insanity) hallucinations were the chief thing noted, with sharply defined delusions, here and there running into a system, while in the second group (simple confusional insanity in old people) the patients were simply confused and disturbed, hallucinations being absent. They were unoriented, did not know what had happened to them, were irritable, sometimes aggressive. They were all old, but not of the specific senile form. The same conditions are seen in exhausting conditions in youth and in alcoholics. All men, from 57 to 77. Cataract operations in all. Psychosis developed soon after operation; in none after sixth day. Unrest, anxiety, aggressiveness showed itself in the beginning. Prognosis not unfavorable. Of the last group there were only three cases. In all inanition, delirium and fatal termination.

Regarding the casual nexus the simplest explanation would be to put the cases among the psychoses following operations, as first pointed out by Dupuyten, and in Germany by Wunderlich. There is no reason to doubt this casual nexus. Can call these affections nothing more than a specific symptom-complex, as Dupuyten has done. They generally take the course of hallucinatory confusional insanity. Rose thinks almost all psychoses following operations are to be considered as delirium tremens, and further that sepsis and high fever may form a substratum of the mental disturbance. Some psychoses developing with hallucinatory confusion he designates as inanition deliria. Von Frankl-Hochwart considers these psychoses relatively rare compared with those following eye-operations. In Vienna the last are much more common than the first.

Winiwarter speaks of psychoses after surgical operations as being especially rare. When Fuerstner reported the first case of insanity after a gynaecological operation he expressed surprise that the case should be so rare while they are so frequent after eye-operations.

Werth could collect only 34 cases of insanity after surgical operations. This disproportion is all the more striking when it is considered how many factors enter into surgical operations that seldom occur in eye-operations, such as the great pain before and after the operation, febrile phenomena, cachexia of cancer, disposition to tuberculosis often occurring in joint disease, inanition, etc. Surgical cases are often depressed in emotions, since they are to suffer the loss of some member, while eye-patients *per contra* have the hope of regaining their sight. The author collects 19 cases of psychoses after surgical operations, of which 3 were of delirium tremens, showing that insanity after surgical operations is comparatively rare in spite of the fact that besides the operation the other important etiological factors are so frequent. What is the special feature of eye-operations that psychoses so often follow them? That lesion of the sensitive optic nerves must be a tremendous irritation is clear *à priori*, and attention is called to the connection between irritation of the trigeminus (neuralgia) and psychoses. Psychoses have developed through simple injury to the bulb—(Griesinger, Arndt and Fürstner). That mental disturbances may arise through irritation of the sense-organs is indicated by the influences of ear diseases and by Esquirol's observation of insanity following a strong smell. According to our author's researches blind people have a special predisposition to mental disease, of predisposing moment in eye diseases is the psychical factor that loss of sight is especially feared [This does not agree well with author's previous statement that hope of regaining the sight was in the favor of these patients as against the fears of ordinary surgical cases]. Also in any of the cases that the oculist has to do with are of advanced age. Of greatest importance, however, appears to be the influence of darkness that is necessary in the after treatment, together with the absolute rest and the separation from the outer world.

Eine psychische Störung combinirt mit multipler Neuritis. (*Psychosis polyneuritica seu cerebropathica psychica toxaemica.*) DR. S. S. KORSAKOFF. Allg. Zeitschr. f. Psychiatrie, 1889, xlvi. Bd., H. 4, p. 475.

Previous to the present article Korsakoff has published articles in Russian describing the disease, which he claims is little known to physicians, although numerous instances have appeared in the practice of alienists and also of gynæcologists. The disease is especially liable to develop after certain diseases, such as puerperal fever, acute and chronic infectious diseases. Korsakoff claims that this form of mental disease is unknown, and that there is no description of it in literature. In almost all cases the symptoms of multiple neuritis may be found, in some cases they are but little marked, in others the symptoms of neuritis, paralyses, contractures, muscle atrophies and pains are so predominating that they may cover up the mental disturbance. Besides the combination with the neuritic symptoms, the symptom-complex of the mental disturbance is in itself characteristic, especially the disturbance of memory and of the association of ideas. All these things taken together give the disease so peculiar a stamp that it is incomprehensible to Korsakoff that it has not been described before, but he explains this by the fact that the disease occurs in the course of other diseases, and the attention of the physician is concentrated on these, and thus the complications on the side of the nervous system are overlooked. The beginnings of the disease are frequently difficult to recognize. Since it ordinarily develops as a complication of severe diseases such as typhoid, puerperal fever and the like, its initial symptoms are bound up with the usual weakness, exhaustion of the nervous system and anæmia of the brain. The beginning is usually ushered in by vomiting, sometimes very stubborn. Then considerable weakness develops. The patient staggers on walking, his gait is progressively unsteadier, finally he can no longer stand and must lie down. The paralysis of the lower extremeties now becomes noticeable, and the motions of the feet and toes are disturbed. The upper extremities, hands and fingers, are also frequently involved. Pains develop in the arms and legs, the muscles fall away considerably, the electrical contractility diminishes, contractures and sometimes œdema develop, and the patellar reflex ordinarily disappears early. In severe cases there may be complete paralysis of the extremities, the muscles of the back become paralyzed, likewise the bladder and diaphragm, and finally paralysis of the heart occurs through disturbance of the functions of the vagus. Parallel with these symptoms in which the multiple neuritis shows itself, there proceeds the development of the mental disturbance. These are less striking in the beginning and manifest themselves externally as simble irritibility or lowered activity of the nervous system referable to the general weakness. At first the patients appear very capricious and assuming, or on the contrary, very apathetic, and sleep in the way that much exhausted men are accustomed to do, but symptoms develop later that make it certain that the disease is not like an ordinary nervous weakness. These symptoms appear either in the form of excessive irritability and great unrest, or as outbreaks of acute mania with clouded consciousness, or again in the form of marked loss in the mental sphere and deep disturbance of memory. Careful examination of the mental symptoms reveals a multitude of peculiarities which are very significant for the diagnosis of this disease. The mental symptoms do not make their appearance in all cases in the same manner. In certain cases there is a greatly increased irritibility and excitability with consciousness well preserved; in other cases, on the contrary, consciousness may be confused, and there may be apathy or agitation, and finally in still other cases a characteristic disturbance of memory comes to the front, a special kind of amnesia. If the mental excitement consists in increased susceptibility and irritability, this is generally displayed in great excitability, unrest and vague fear. The patient fears death, an attack, or he knows not what; fears to remain alone, constantly calls to himself, sighs, or laments his fate. Not rarely the consciousness remains clear a long time, but in many cases after the first few days of excitement consciousness becomes confused. Patient mixes up words and cannot speak connectedly. Every day the confusion increases, patient begins to tell of all kinds of monstrosities, speaks of journeys that were never performed, mixes up old reminiscences with recent events, does not know where he is or what is going on about him; sometimes illusions of sight and hearing develop which still more confuse the patient. Thus the same patient is at times entirely quiet, at other times very restless. The disturbed periods usually come on towards evening, when the patient begins to be restless, becomes angry if he is not given what he wants; sometimes the restlessness reaches a very high degree. There may be attacks of raving, of acute mania. Sometimes these may occur in the beginning of the disease, later the excitement may still exist, yet it may not break out in attacks, but is limited to singing songs the whole night through. Sometimes the disturbance of consciousness reaches a very high degree and may almost go to the complete loss of consciousness. With this there also goes a deep disturbance of memory. It takes the form of a peculiar amnesia, in which the memory for recent events is principally disturbed, while that for events long past remains very good. Generally such an amnesia develops after the excitement already described, with confusion of consciousness; this excitement lasts some days, then the patient becomes quiet, and his consciousness becomes clear, he begins at the same time to gain back his mental faculties, but his memory remains deeply disturbed. This especially shows itself by his asking the same question and repeating the same things. In the beginning the presence of a mental disturbance is hard to recognize in conversation; he gives the impression of a man who is complete master of his mental faculties, draws correct conclusions from given premises, plays cards and chess, in short, conducts himself like a mentally sound man, and only after a long conversation can one notice that from time to time the patient mixes up matters in an extraordinary manner, and does not remember what goes on about him, does not remember whether he has eaten, whether he has been out of bed. Many times the patient immediately forgets what has happened; some one comes to him and speaks to him, goes away for half a minute, and on his return the patient has no recollection that he has been with him. He may read the same page for an hour and have no recollection of what he has read. He may repeat the same things twenty times without being in the least conscious of the constant repetition of the storeotyped phrase. He cannot remember the persons with whom he comes into contact exclusively at the time of his sickness, although he sees them constantly, and every time he sees them he is sure that it is the first time.

The phenomena in which the amnesia is manifested differ in some degree according to the degree of the disease and the intensity of the disturbance. In the slighter degrees the memory for the more recent past is not completely lost but the events remain only vague, floating in memory. Often the patient recollects the affair itself but not the time when it took place; in other cases the forgetfulness concerns the peculiar thought-processes, in consequence of which the patient does not know what he has said, and continually asks one and the same question. Sometimes all the facts are present in memory, but the patient needs special conditions to bring them to consciousness.

On the other hand, in very severe cases the amnesia is much deeper, and the recollection is lost not only for recent events but also for earlier ones; it especially happens that the present momentarily disappears out of the patient's memory while events of years ago come to the front, and the patient mingles old reminiscences with new impressions of the present; he thinks himself in the same conditions as thirty years ago, and the persons about him to be those whom he knew at that time, who, perhaps, have long been dead. In the more severe forms the memory for events is completely lost, and even the word memory disappears; the patient forgets his own name and brings out unconnected sounds instead of the words. With the severe forms of amnesia there also ordinarily occurs a marked clouding of consciousness, which in the severest cases may amount to a condition of complete loss of sense. The amnesia has no stationary character, it may be greater or less. The variations in its intensity depend among other things on temporary conditions; by fixing the attention of the patient and securing his good will the memory is often better. Most frequently, however, the intensity of the amnesia naturally depends on the general course of the disease and on the depth of the general disturbance. Thus the amnesia diminishes on the improvement of the disease, and may entirely disappear; but if the disease becomes worse the amnesia becomes deeper and deeper, and in addition to the symptoms of the amnesia a marked confusion is developed. This confusion comes out in slight degrees in this form of amnesia, but the confusion is not with regard to the impressions that the patient receives at the moment but only with the earlier events. To the question how he passes his time the patient often does not answer at all what is the case, but replies that he went to the city yesterday, whereas he has not left his bed for two months; he tells of imaginary visits, conversations, etc.; sometimes such patients invent a story and repeat this continually, so that a peculiar form of delirium develops having its root in pseudo-reminiscences.

These are in general the most characteristic features of the mental disturbance observed in patients suffering from this disease. By the side of the mental symptoms there go, as already said, the ordinary phenomena of degenerative multiple neuritis, such as paralyses of the lower, and sometimes also of the upper extremities. These symptoms are not always clearly defined; in many cases they are only indicated by insignificant pains in the legs and unsteady gait. The patellar-reflex does not entirely disappear, but is frequently increased, or remains normal. In addition there can always be found somewhere on careful examination the signs of neuritis, which thus assists in the diagnosis of the psychical disturbance. Besides the phenomena of neuritis there also ordinarily exists in this disease disturbances of the general organism. There is much emaciation, very frequently severe vomiting, diminished excretion of urine, which on this account appears reddish brown like strong tea. The phenomena of myositis are not rarely present. Sometimes the heart's action is disturbed and the pulse is irregular; at times dropsy develops; in women the menses stop; lower temperature develops. Besides the neuritis and the symptoms of disturbance in the hemispheres, other phenomena related to the brain and cord not rarely develop, such as disturbances of speech and swallowing, and sometimes opathalmoplegia externa, nystagmus and the like.

The course and termination of the disease depend on its intensity and the conditions under which it has developed. As already stated, the disturbance often comes on in the course of other diseases, acute or chronic; it is also not at all rare in chronic alcoholism, as well as common in the different intoxications. Not infrequently, for example, in alcoholism the disease may set in with symptoms which are entirely similar to delirium tremens, and subsequently there are joined to this the paralyses and the characteristic disturbance of memory. A similar beginning not rarely comes on in the course of puerperal diseases; an attack of panphobia suddenly breaks out with intense excitement, followed by confusion of consciousness, failure of memory and other symptoms.

In other cases in very weak patients the disease comes on unnoticed, without a sharply marked attack; a gradually increasing forgetfulness develops, and then confusion of consciousness is added to this, reaching the highest degree. The termination of the disease depends equally on its intensity and its mode of origin.

If the source of the disease is removed the termination is not infrequently a favorable one, recovery may set in, generally after a very long time, after several months, still oftener after some years. If, on the contrary, the source of the disease is not removable, if it is, for example, a disturbance that has developed on the basis of a tuberculous or carcinornatous cachexia the termination is for the most part an unfavorable one. The disease may also proceed to a fatal termination if it develops with great intensity in an organism which has only slight resistive power. Thus the beginning, course and termination of the disease, stand in the most immediate relation to the etiology. The etiology is the same as that of multiple neuritis, and all the causes which may bring on multiple neuritis lead at times also to this form of multiple disturbance. As multiple neuritis comes on with special frequency in drinkers so this form of disease comes on very frequently in alcoholic neuritis and alcoholic paralysis.

The mental disturbance above described has received some attention by different writers, first by Magnus Huss, but no one saw in the mental disease anything peculiarly connected with the neuritis, but all held the psychosis to be simply a complication of the disease under the influence of alcohol. Korsakoff claims to have been the first to show that a completely analogous mental disturbance develops in cases of multiple neuritis where alcohol can play absolutely no rôle as an etiological factor, and he has published fourteen cases of multiple neuritis of non-alcoholic source with a clearly marked mental disturbance. These observations lead Korsakoff to conclude that this mental disturbance belongs to multiple neuritis and to ascribe its origin to the influence of the same pathogenic character which produce multiple neuritis. These conditions do not always appear to bring on the mental disturbance in the same degree as the neuritis, for in many cases the neuritic symptoms appear more marked because the pathogenic agent has worked more on the peripheral nervous system, while in other cases the mental symptoms predominate in consequence of the pathogenic agent influencing the brain by preference. In still other cases the cerebral and peripheral disturbances are marked in almost equal manner.

Turning to the etiology of the fourteen cases published by Korsakoff we find the sources of the disease to be very different, such as the presence of a dead fœtus, puerperal septicaemia, accumulation of feces, typhoid tuburculosis, diabetes mellitus, lymphadenoma, and the breaking down of a tumor. Adding to these that this form of disease also develops in alcoholism, poisoning with arsenic, lead, sulphuric acid, carbonic oxide, etc., we see that the sources of the disease are extremely varied. Still it is easy to see that there is something in common in them all, since in all these cases the composition of the blood is altered poisonous substances are accumulated in the blood, and it is in the highest degree probable that it is these which poison the nervous system, in individual cases the peripheral nervous system being puerperally affected, in other cases the central nervous system, but often both in the same degree. It is hard to say what these poisonous substances are, but in most cases they belong to the ptomaines or leucomaines, which have reached the organism from the outside or have developed in it under favorable conditions. Korsakoff has very properly named all of them *toxaemic cerebr opathies* (cerebropathie psychica toxæmica). They may also be called *polyneuritic* psychoses (psychocis polyneuritica), but it must be borne in mind that cases of this kind of mental disturbance may develop in which the symptoms of multiple degenerative neuritis may be poorly marked and thus may be overlooked. The pathological anatomy of the disease is still not sufficiently explained, but the presence of multiple degenerative neuritis may be looked on as proved.

Ueber eine besondere Form psychischer Störung combinirt mit multipler Neuritis. S. S. Korsakow, Arch. f. Psych., 1890, xxi Band, 3 Heft. p. 669.

The present article is mostly taken up with a consideration of the etiology of multiple neuritis, and the author refers to the fact that in the beginning of the year 1887 he advanced the theory that in addition to the poisons that get into the body from the outside and cause neuritis, this may also arise from poisons developing in the body itself—ptomaines and leucomaines. The views of Rosenheim and Leyden on the origin of multiple neuritis are given, together with those of the French authors, Bouchard, Charin and Roger.

In any disease where the eliminative powers of the body are reduced we may get auto-intoxication from the accumulation of the ptomaines and leucomaines, multiple neuritis, and together with this Korsakoff's *cerebropathia toxæmica*. This has developed in glycosuria, in pyæmia, in tuberculosis, in pyæmia, and after typhoid, after the birth of a fœtus that had undergone decomposition; in this latter case there were absolutely no phenomena of putrefaction to be found on the genital apparatus but the disease had apparently developed directly through absorption of ptomaines in the blood. In the cases cited numerous instances are given which point to the abnormal constitution of the blood; one case developed in connection with leucocythæmia, another in a liver disease, a third with the breaking down of a neoplasm. Korsakoff would ascribe to the ptomaines or leucomaines resulting from the activity of the tubercle bacillus in tuberculosis the physical disturbance so frequently found in this disease, contrary to the view of Wood who would account for the disease simply by the great exhaustion produced. In view of all these facts Korsakoff calls the cerebropathy described a toxæmic cerebropathy, since he assumes that all cases of this disease stand in connection with some one toxæmia. In individual cases the fundamental toxæmia influences the peripheral nerves alone, in other cases it affects the cord, and in still others the brain. These latter cases being the ones in which the mental disturbance is produced. Why in the one case the affection is confined to the peripheral nerves while in another case the brain is a fellow sufferer is unknown. Apparently this depends on the affinity of the poison circulating in the blood, and in part on the dissimilar powers of resistance of the nervous system in different men. The fact that physical disturbance in question has been observed to be especially frequent in multiple neuritis of alcoholic origin may well be conditioned on the fact that the brain has become particularly susceptible through the drinking of alcohol.

The nature of the poison circulating in the blood also apparently has something to do with this difference, for while there is almost always a disturbance in alcoholic multiple neuritis, yet in the neuritis after diphtheria there is no known case where a psychosis has developed.

In his earlier work on alcoholic neuritis Korsakoff explains this excessive vulnerability of the brain through an apparent alteration of the lymph apparatus in general, and especially of the connective tissue, this alteration establishing itself in the nervous system in alcoholism, and in consequence each accumulation of toxic products in the blood or lymph leads much quicker to poisoning than in normal conditions. This explains why multiple neuritis and cerebropathies are especially frequent in the tuberculosis of drinkers, and also why in such cases neuritides and cerebropathies break out in consequence of strong emotions or marked physical exhaustion, the products of fatigue in such cases are not sufficiently eliminated through the lymph and act toxically on the nerve elements. If this is the case, then the designation of such forms of disease as *toxaemic* is not strictly correct since the direct source of the disease is to be looked for not in the blood but in the fluid saturating the tissue elements. In this appears to Korsakoff to lie the real objection to the name adopted by him, yet in default of another the title *cerebropathia psychica toxaemica* seems justified, and to characterize the disease and its genesis.

The article contains the minute clinical reports of six cases. The first that of a woman who gave birth to a dead child in which decomposition had already set in; secondly, an analogous case, the child being healthy but the after-birth being retained; in the third case psychosis followed typhoid; in the fourth case there was specific disease, abuse of alcohol, malaria and lymphadenoma; in the fifth, probably retrogressive metamorphosis of a fibroma; in the sixth the etiology was doubtful, although alcohol may have had some influence.

The six cases were observed in two years. The first, third and sixth cases recovered, the other three died.

Ein Fall von polyneuritic Psychose mit Autopsie. S. S. KORSAKOFF UND W. SERBSKI, Arch. f. Psych., 1891, xxii Band, 1 Heft; 112-134.

The psychosis in this case followed a laparotomy for the removal of a dead fœtus in a case of extrauterine pregnancy. A septic fever developed before the operation, after which the temperature fell perceptibly, although it always remained high.

A week after the operation, in addition to the irritability manifested earlier, there was considerable excitement and a clearly marked weakness of memory for recent events. Consciousness was clear in the beginning, but soon began to be clouded, and at the same time symptoms of weakness in the extremities developed, the tendon reflex disappeared, and the symptoms of multiple neuritis developed.

Although the wound healed the affection of the nervous system increased; the disturbance of memory became more marked, the association of ideas was completely lost, from time to time there was excitement, and hallucinations developed. The paralysis increased, and extended to the upper part of the body, and the patient died from paralysis of the diaphragm. As in the previous cases Korsakoff attributes the disease to the poisoning of the central and peripheral nervous system by the ptomaines circulating in the blood. At the autopsy the characteristic degenerative changes of multiple neuritis were found. The phenomena of multiple degenerative neuritis were found in all the nerves examined with the exception of some cranial nerves. The muscles showed evidences of a degeneration of an irritative character—increased number of nuclei. In the brain nothing was found by the methods used, but Korsakoff thinks that the failure to find any changes in the brain was to be accounted for by the fact that the mental disturbance had existed in the patient only a relatively short time, and that the anatomical substratum of the disturbance did not have time to develop to a sufficient degree to become evident by the methods of investigation employed; possibly also because the cortex was not examined by all the methods.

Korsakoff does not think that the negative result justifies the assumption that the mental disturbances in multiple neuritis is unaccompanied by any changes in the cortex, but he is much more of the view that these changes exist in many cases, and cites as a proof that in his observations on alcoholic neuritis where a characteristic mental disturbance was present a change in the cortex was found, viz: alteration of the vessels, miliary extravasations, increase of the connective tissue and spindle cells.

Polyneuritis und Geistesstörung. ERNST FRANK. Inaugural Dissertation, University of Bonn, 1890.

Frank reports a case of mental disturbance, to which the phenomena of polyneuritis were added very early. The clinical picture is very similar to the psychoses described by other authors as occurring in multiple neuritis, although some of the symptoms usually present in these psychoses were absent in this case. The author quotes Korsakoff's description of the mental condition. Frank's case presented especially the peculiar disturbance of memory described by Ross. While in almost all cases of psychoses in multiple neuritis, as described by Korsakoff and others, there are still other phenomena, such as delusions, hallucinations, illusions, stupor, and even well-marked delirium tremens, yet these according to Frank, only develop in alcoholic multiple neuritis, or in those cases which are due to infection or other form of intoxication. The question arises whether it is necessary to look for the origin of such disturbances in a pathological and anatomical change in the brain, as has been done by many, or if the outbreak of psychoses in multiple neuritis may be explained without such an assumption.

Tilling holds that such a direct and anatomically provable disease of the brain exists in consequence of the same injurious conditions which affect the peripheral nerves. Tilling's explanation, according to Frank, holds good only of cases of alcoholic polyneuritis, and whether in such cases such an explanation of the connection between psychical disturbance and mental disease may be disputed; at all events, autopsies made up to this time speak against this. Spinal changes, at least such as would correspond to the clinical phenomena, have never been found, not even in cases of alcoholic ataxia, the so-called alcoholic pseudo-tabes. On reviewing the evidence advanced by different writers Frank comes to the conclusion that such cases present no anatomically demonstrable lesion of the brain, but that the psychosis depends on such disturbances of the central organ as are usually called functional, in which with our present means of investigation no anatomical change in the brain is demonstrable. With regard to the question of Beri-Beri the author draws the generally accepted conclusion that such cases are due to infection. After a general review of the literature Frank concludes that his own case of polyneuritis without alcoholism, infection or intoxication is the sole one of the kind in literature. The psychosis was, however, characteristic throughout, and in its individual phenomena not less intense than those cases of psychoses developing in polyneuritis on an infectious or toxic basis. The etiology is sufficiently explained by the poor conditions of life to which the patient was subject for a year before the attack. Frank claims that his case shows that polyneuritis with mental disturbance may develop without one being able to allege as a cause either an infection, or even a special disease — the "cerebropathie psychica toxaemica," and that the pathological findings up to this time afford no special explanation of the psychosis in a primary pathologico-anatomical change in the brain. It results therefore that it is not simply toxaemic influences to whose influence on the peripheral nervous system polyneuritis owes its origin, and that in his case any such source, as well as epilepsy, senility and trauma must be excluded, and the only source to be sought is in the poor manner of living, which together with the small and minute injuries to the peripheral nerves is sufficient to call out the disease.

On the Psychical Disorders of Multiple Neuritis. JAMES ROSS. Journal of Mental Science, April, 1890.

Except in a few idiopathic cases multiple neuritis is due to the action of some poison,—diphtheria, septicemia, typhoid and other fevers, syphilis and tubercle; vegetable poisons like morphia; diffusible stimulants,—alcohol, bi-sulphide of carbon, di-nitro benzole, and the fumes of naphtha and other agents used in special manufactures; endogenous poisons, like those generated in rheumatism, gout and diabetes; metallic poisons, lead, phosphorous, arsenic and mercury. Multiple neuritis also accompanies many diseases like cancer, Addison's disease, exothalmic goitre, chorea, chlorosis, hæmoglobinuria, pernicious anæmia, and other diseases attended by great impoverishment of the blood. Some degree of neuritis also probably follows after severe shocks to the nervous system from injuries or moral causes. Whatever the cause of this form of neuritis it is likely to be attended by psychical disorders which have in all cases a certain family likeness; the best marked examples are in the poisoning by morphia, alcohol and other diffusible stimulants.

Ross divides the psychical disorders of multiple neuritis into four stages: First, a premonitory stage, in which the special senses and the imaginative faculties are likely to be exalted; second, a stage of depression or melancholia; third, a transition to mania or melancholia with excitement, or of convulsions, passing on to, fourth, a final stage of dementia.

In the stage of exaltation the patient often suffers from faint hallucinations. A patient with glycosuria on closing his eyes saw all sorts of figures passing before him, such as soldiers and policemen in threatening attitudes; heard music on several occasions. In a case of alcoholic paralysis in a man of 21 when he closed his eyes a bright cloud shone before him and in the midst of it appeared faces which he spontaneously compared to photographs. In this stage there is unreasoning irritability of temper and suspicious disposition. A case illustrative of the melancholic stage was characterized by gloom, sleeplessness, mental agitation, restlessness, vivid but corrigible hallucinations in full light, and in this stage alcoholic cases find a necessity of taking stimulants for taking stimulants on going to bed. When this stage is reached the mind is apt to be chased by a tumultuous tempest of conflicting thoughts and passions which altogether prevent sleep. Ross thinks acute delirium comes on very readily when such melancholic cases begin to indulge in drink; others develop excitement or mania; while a third group manifests certain incapacities for business and are rendered unfitted for attending to their social duties. Such patients become shy and retiring, and cease to mingle in society. This timidity is seen in females who give way to secret drinking, early cease to attend to social duties, refrain from visiting, and their friends find them indisposed. As the disorder increases they become distrustful and suspicious of nearest friends, often accuse their neighbors of circulating scandals about them, or of overt acts of insult. Patients in this stage suffer from dizziness, a feeling of insecurity in walking, and a peculiar disarrangement in their perception of the space relations of surrounding objects, which may be regarded as a hallucination of the muscular sense. Ross quotes De Quincy with regard to this peculiar prolongation of the sense of time and space. In the melancholic stage the patient often suffers from remorse for some past act, often foolish, is timid and filled with thoughts prompting him to commit evil actions. These thoughts often take an erotic turn while at other times they assume the form of suicidal impulses. For the third or maniacal stage Ross refers to Bevan Lewis's text-book. In this stage there are visual hallucinations, vivid and incorrigible, burglars, detectives, men in collusion with their wives, etc. Aural hallucinations now assume the form of distinct voices uttering blasphemous oaths and curses, or are voices of ill-disposed persons intriguing against the patient, or they become commands from heaven or threats from the spirits of darkness. The delusions connected with the lightning like pains and other sensory disorders which the patient suffers are endless.

The last stage of alcoholic insanity is alcoholic dementia.

Ross's description agrees with that of Korsakoff as regards patients stating that they have been out walking, etc., when they have not left their bed.

Toxic Insanity Especially in Relation to Chronic Alcoholism. S. A. GILL. Medical Times and Circular, May 21, 1890.

Gill defines toxic insanity as caused by the presence in the circulating blood of such poisons as alcohol, opium, chloral, uric acid, lead, and the like. Discusses only alcohol in its remote effect on the nervous system. Divides alcoholic insanity into acute and chronic. The former is mania à potu, melancholia à potu, and delirium tremens. Does not discuss these, but simply calls to mind whether the symptoms they present are found in chronic alcoholism or chronic delirium tremens, as Maudsley calls it. The symptoms are slow and gradual in their development, yet are preceded by the same premonitory signs. It is popularly thought that whenever the mind gives way from alcoholic excess that delirium tremens must result; this is erroneous as there are hundreds of alcoholic subjects who never have delirium tremens, yet slowly and surely develop nervous symptoms that bring them within the walls of an asylum. No general description of the mental condition in such cases is given; a case of chronic alcoholism is described and the pathology of this disease is given.

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ON CERTAIN PECULIARITIES OF THE KNEE-JERK IN SLEEP IN A CASE OF TERMINAL DEMENTIA.

FROM THE LABORATORY OF THE McLEAN ASYLUM, SOMERVILLE, MASS.

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In the more precise investigations of the physiological conditions modifying the knee-jerk all investigators have found that the mental condition of the normal subject entered largely as a disturbing factor, the different emotional states and the varying conditions of the nervous system invalidating the result to a certain extent, or at least rendering necessary an enormous number of observations before the "normal" knee-jerk for any individual could be obtained. Emotional states being almost completely absent in cases of advanced dementia it was thought that an investigation of the knee-jerk in this state might yield some results on the conditions modifying it. An excellent subject was found in an elderly man, admitted to the asylum in 1841, and with the exception of the seven years preceding 1850 having spent this whole period at the McLean Asylum.

In 1843 it is stated that his "mental faculties [are] mostly gone; never makes an inquiry; spends his time in wandering about the yard; slovenly in his dress;" and but little or nothing could be added concerning his mental condition at the present time. His dementia is complete, he having absolutely no knowledge of his surroundings, not even knowing his name, is unable to answer questions relevantly, his talk being utterly incoherent. He is good natured and docile and has never made the slightest objection to the experiments, which almost always mean an extra nap for him. In fact he will go to sleep in almost any position in which he is placed, and remains perfectly quiet and contented. Several demented men were tried, but with the exception of this one they all proved resistive or restless, and were given up one after another. The patient finally selected was a peculiarly good one for a prolonged series of tests on the condition of his nervous system as he was subject to periods of depression and exhiliration, such as not infrequently occur in chronic dements, and a record was made of all his bodily functions, pulse, temperature, weight, blood pressure so far as this could be measured, mental condition, and finally knee-jerk in the hope of finding some change in his bodily condition coincident with the change in his mental state. Unfortunately, from the point of view of gaining any information on this particular point, after passing through one cycle of exhiliration and depression he settled down into a state of comparatively even mental and physical life, and as the observations taken during this cycle would need further confirmation no mention will here be made of them. The original problem, also, the theoretically lesser variability of the knee-jerk in a demented person, gradually changed into another as will appear subsequently, and the present paper will be confined to a consideration of two important points that came out in the course of the investigation.

Apparatus and Method of Experimentation.

After a trial of different hammers that of Lombard[1] was finally adopted, that of Prof. Bowditch[2], which was kindly loaned for an experimental trial not proving strong enough for this patient in whom a rather heavy blow was necessary. The hammer was of cast iron, the handle being an iron rod 20 cm. long, on the end of which a similar piece of iron rod 4 cm. long was fastened horizontally, serving as an axis and supported in an iron frame work. An index on the handle moved over a graduated scale, so that by raising the hammer any given number of degrees the same strength of blow was given. Throughout the experiments the hammer always fell 45°, except in a few instances where the strength of the blow was changed for a short time for some special purpose. The hammer was supported by horizontal rods and clamps to a piece of gas-pipe, serving as an upright support fastened to a firm wooden base that could be securely clamped to the table. The arrangement combined firmness and strength, while at the same time the hammer could be quickly raised or lowered, or moved in any direction horizontally, and thus easily adjusted to hang at zero of the scale, when the patient's leg was put in position. When pulled back into position to strike the blow the hammer was held by a large electro-magnet, the support of which was also adjustable. The hammer was pulled back by a cord and released by a simple circuit-breaking key. The blows were always given at 5 seconds intervals, except that the interval was occasionally changed for special purposes, but in the reports of the experiments 5 second intervals are always to be understood unless otherwise distinctly stated. The patient reclined on his right side on a mattress placed on a table, with a firm support extending the length of his back. The left knee was supported on a wooden arm extending from a wooden upright that was firmly clamped to the table, and the left foot was supported in a stirrup hanging from the ceiling allowing free movement of the leg. Attached to the stirrup at the point on which the heel rested was a steel rod 3 feet long, passing backwards and supported on pulleys; on its further end was fastened a thread passing over pulleys and attached to a short vertical steel rod, moving up and down through two brass supports, and suspended from above by a light spring. A cork placed on the rod carried on its side the writing point, a piece of light stiff celluloid, which pressed lightly against the drum of a Baltzar Kymograph. When the blow was struck the foot moved forward pulling the steel rod with it, and this by its thread attachment pulled down the writing point, leaving the record of the full length of the kick on the smoked surface of the paper. There was of course always a slight backward kick which appears in the records above the horizontal line made by the drum revolving under the writing point when this was at rest.

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[1] *American Journal of Psychology*, 1887, Vol. I., p. 12.

[2] Bowditch and Warren *Journal of Physiology*, 1890, XI, p. 27.

Disappearance of the Knee-Jerk in Sleep and the Effect of Auditory Stimuli.

The disappearance of the knee-jerk in sleep has been previously observed.[1] Its complete disappearance was early noticed in our patient and this is therefore no departure from the normal. As would naturally be expected the patient went to sleep much more easily than a person with an active mind, and it was thus possible to observe the phenomena a large number of times.

The behaviour of the knee-jerk when sensory stimuli are received by the patient during this period of sleep appears, however, to be a distinct departure from the normal, and suggests that the weakened state of his brain permits the sensory stimulus to exert its effect in reinforcing the knee-jerk for a much longer period than in a person with a sound brain.

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[1] Bowditch and Warren, loc. cit. p. 59.
Lombard, loc. cit. p. 53.

That the knee-jerk is increased when the patient clinches his hand or makes any violent movement coincident with the blow was first shown by Jendrássik[1]. "He also thought that stimulation of the sensory nerves had a similar influence on the tendon reflex, but considered his experiments on this point incomplete and that such an influence was more difficult of determination." (Quoted from Bowditch and Warren.)

Mitchell and Lewis[2] made a study of the conditions under which the knee-jerk is increased and diminished, and found that volitional acts directed to other parts of the body, painful stimulation of the nerves of the skin either by pinching or by the application of heat, cold or electricity, caused a reinforcement, as did also a burning magnesium wire exposed to the eyes.

Lombard found that sensory irritations, voluntary movements and strong emotions when synchronous with the blow increased the knee-jerk. The investigation of Bowditch and Warren had for its object a study of the exact relations in time between the knee-jerk and the reinforcing act, and was suggested by the statement of Mitchell and Lewis "that the muscular action or circuit closing, must precede the tap, in order to reinforce it, by a period which is, as yet, undetermined." The conclusion of Bowditch and Warren with regard to auditory stimuli was that "the effect of a sudden auditory stimulus on the extent of the knee-jerk was, in the three subjects of experiment, almost wholly positive, though great individual differences were observed. The maximum effect was produced when the interval between the sound and the blow was 0.2″—0.3″."

As the results here to be recorded have to do with the knee-jerk in sleep the experience of previous investigators on this point is of interest.

Bowditch and Warren[3] found that "the monotonous character of the experiment was often found to produce a decided tendency to sleep in the individual experimented on. To counteract this tendency and to insure a certain degree of attention to the phenomena, the subject of the experiment was required to declare after each knee-jerk whether or not a sensory-stimulus (i. e. sound, flash, etc.,) had been perceived, or in other words, whether the knee-jerk was normal or reinforced. In spite of this precaution the tendency to sleep was sometimes quite irresistible, and in eight or nine cases the experiment was continued after the subject had yielded to it and was sleeping soundly. It was then found that the knee-jerks, both normal and reinforced grew gradually smaller, and when sleep was profound, disappeared entirely, the blow upon the knee being absolutely without effect. This result is not what might have been expected from our knowledge of the effect of sleep on the ordinary cutaneous reflexes, e. g., that produced by tickling the sole of the foot. Whether this can be regarded as an argument against the reflex character of the knee-jerk, or whether we have here an essential difference between deep and superficial reflexes, are questions to be decided by future investigations."

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[1] Jendrássik: Beiträge zur Lehre von den Sehnen reflexen. Deutsches Arch. f. Klin. Med. 1883, XXXIII, 177.

[2] *Medical News*, Feb. 13 and 20, 1886.

[3] Loc. cit. p. 58.

Prof. Lombard has kindly gone over with me his own curves made in 1887 in New York, generally on healthy medical students, and many of these show an absolute disappearance of the knee-jerk in sleep, and they also show a sudden rise from 0 from some accidental stimulus such as the entrance of a person into the laboratory, but this effect lasted over but a few kicks.

In our demented patient when the knee-jerk has entirely disappeared in sleep an auditory stimulus causes an increase in the length of the kick, and this increase is visible over a long series of jerks. This latter phenomenon is apparently a departure from the normal and a peculiarity of this subject. This in well shown in Fig. 1. The patient was asleep from the beginning of the experiment and soundly asleep with total disappearance of the knee-jerk during the minute and three quarters preceding the time when two light taps were given on the table with the wooden handle of the needle that was being used by the observer to make records on the drum.

The knee-jerk rose from 0 to 9.5 mm.; in 5 seconds rose to 39.5 mm.; in the next 5 seconds to 59 mm.; then fell to 38 mm.; through the 29 succeeding kicks gradually fell with slight fluctuations to 6.5 mm.; remained at practically this length for 3 kicks; rose to 15 mm., and then to 28 mm.; fell to 16 mm. and 14 mm.; then came 5 kicks averaging 6.6 mm.; then came a rise without apparent cause to 44 mm.; then followed a fall to 0 after fluctuations extending over 27 kicks. Thus one auditory stimulus had an apparent effect extending over 72 kicks occupying 6 minutes. During this whole time the subject was sleeping soundly, being apparently as sound asleep as when the auditory stimulus was first given. Five kicks of the value of 0 then followed, and there then came on another series of increased kicks with no apparent cause, lasting over 16 kicks, with another total disappearance. There then followed 28 kicks with almost total disappearance of the knee-jerk and then two taps (B) brought out the series of reinforced kicks again, with a peculiar series of groups of kicks extending to near the end of the experiment, with occasional total disappearances. Toward the end (C) two taps produced almost no effect, causing a rise only to 10 mm. from a preceding 7, then followed 18, 10.5, 4 and 0. After three of 0 value another lengthened group came on with no apparent cause, and at this point the subject was awakened by being spoken to. It was noted that this day he was decidedly more dull than on the day before, that he slept from the beginning to the time he was awakened, and that all efforts to rouse him at this point amounted to but little as he would simply mutter a little and then drop off to sleep again. There were no accidental sensory reinforcements that were sufficiently noticeable to be brought to the consciousness of the observer who was on the watch to note them. The peculiar set of "groups" of kicks occurring during the course of this experiment will be discussed later on.

This prolonged effect of a sensory stimulus is seen again in Fig. 2, where two clicks were given on a telegraph sounder at A, after the knee-jerk had entirely disappeared. Here the kick rose from 0 to 18 mm., and then to 42 and 44, falling through 17 kicks to 3 mm., not reaching 0 again for 17 more kicks. There were three kicks of the value of 0, a short rise over 4 kicks, a total disappearance during 2 more, the needle not even making a dot on the line, and then there came another of the "groups," B, with no apparent exciting stimulus, lasting over 10 kicks before these entirely disappeared again. There were slight rises above the 0 line, and at C a noise on the floor below caused another series of reinforced jerks, and the subject awoke. Practically this same effect of stimuli is seen in Fig. 3, where at A and B walking on the floor below caused the rises there shown; the much lesser effect of the second stimulus is seen in the fewer number of kicks that were caused by it. Blows were delivered at the regular 5 second intervals although without the effect of moving the needle from the straight line, until at C two taps with the needle-handle and a distant locomotive whistle a little later caused the prolonged series of increased kicks that followed. Here again it must be remembered that to all outward appearances the patient was sleeping as soundly as when the sensory stimuli were received. Fig. 4 shows the same effect once more, the knee-jerk having entirely disappeared during the period represented by the straight line preceding A, blows being struck at the regular 5 seconds intervals, while at A, a passing barrow caused a slight rise and at B the noise of a passing cart caused a much greater rise, with a secondary rise a little later, with another rise before the knee-jerk entirely reached 0 again, and then after three kicks of the value of 0 there was still another rise, when there was again total disappearance.

In Fig. 5, the effect is shown of giving the rapping-stimulus seven times in succession. The largest effect was after the second time where the reinforcement continued over 13 kicks before the zero point was reached. After the third stimulus there were 8 reinforced jerks before the kicks sank again, not to 0, but to 4 mm. Stimulus no. 4 called out only two reinforced kicks, no. 5 called out 2, no. 6, 4, and no. 7, 6. At no. 8 the patient was awakened.

These would seem to show that the subject had gradually become accustomed to the stimulus and thus its effect was lessened. If it is argued that the patient was more wakeful at 6 and 7 as shown by the fact that the kicks did not return to 0, this would mean also that stimuli did not have so great an effect during this more wakeful condition as during the condition of deeper sleep. This diminution of the effect of a reinforcing stimulus is shown in Fig. 6 where 2 clicks were given on a telegraph sounder at (1), causing a marked effect lasting through 6 kicks, and then disappeared, to be followed by a secondary rise during 7 kicks, and then a total disappearance. A repetition of the clicks however at (2) was followed by no response for the first kick but the second and third kicks rose respectively to 1 and 2 mm., which ordinarily would be accounted for by the jarring of the apparatus, but in this particular instance there had been no movement even from the jarring, so it seems fair to attribute these two movements of the writing needle, slight as they are, to the knee-jerk. A third repetition of the stimulus (3) called out a knee-jerk of 3 mm., followed by a total disappearance during 5 kicks, then a kick of 8 mm. with no apparent preceding stimulus, a disappearance of three kicks, and then a repetition of the clicks, this time three clicks instead of two, produced a kick of 8 mm. with another total disappearance. A repetition of the three clicks (5) caused no response for the first kick, but the second rose to 26 mm., the third to 43, and from this time on there was no disappearance but a kick followed every blow of the hammer.

Attempts to find a similarly marked reinforcement from auditory stimuli while the patient was awake did not meet with as good results as while he was asleep. Fig. 7 shows a portion of a curve taken from the middle of a tracing while the subject was fully awake. At the places indicated by dots two clicks were given on the telegraph sounder and an apparent reinforcement appears at times, and again an apparent inhibition. This disagreement may be due to the fact that the interval of time by which the sound preceded the blow was not measured, so that the blows may have been struck at the reinforcing interval at one time, and at the inhibiting interval at another. The irregularities in the kicks, during the period that the reinforcing signals were being given, are no greater than the irregularities in the kicks preceding and following the reinforcing signals. The sounds both inside and outside the laboratory, that had so much effect during sleep, never appeared to have a corresponding effect when the subject was awake. To have settled this point definitely would have necessitated a repetition of the elaborate experiments of Bowditch and Warren. Nor was the interval by which the sound preceded the blow in sleep measured; it probably usually varied between one and two seconds. As the peculiar prolongation of the reinforcement made its appearance apparently irrespective of the interval by which the sound preceded, particular attention was not given to this point. It would be interesting to determine if an interval could be found at which the stimulus would inhibit such a prolonged series of kicks as shown in some of the curves. On several occasions the first kick after an auditory stimulus did not rise as high as the second, as shown in Fig. 3, and this raises the question whether the inhibiting interval may not have accidentally been struck here, but the effect of the inhibition passing off during the succeeding 5 seconds, the stimulus exerted its full force and the kick rose to the maximum. Should this be so, it would seem to point to the necessity, in measuring the interval by which the blow must be preceded by the sensory stimulus to produce inhibition or reinforcement, of following the first blow by several more at comparatively short intervals. With regard to sensory stimuli that reached this patient during the time he was awake it can only be stated that these appeared to have a very slight and trifling effect compared with those that reached him during sleep.

It should be added that previous observers of the disappearance of the knee-jerk in sleep and of its rise in this condition in response to external stimuli have not delivered the blows at the same intervals as in these experiments, consequently it is possible that some of the effects of the stimuli may not have been recorded in their tracings. Attempts were made to settle this point by experiments on normal individuals, but the knee-jerk did not entirely disappear in sleep in the trials that were made. In Fig. 12 there is shown a portion of one of these tracings from one of the medical house-pupils. The experiment was begun at 10.10 p. m. and continued until 11.15 p. m. The portion shown is from about the middle of the tracing, after the subject had become thoroughly drowsy, and a grdual diminuation of the length of the kicks is seen as the experiment progressed. When it became probable that the knee-jerk would not completely disappear the customary auditory stimuli, two taps with the needle-handle, were given at (1), (2), (3) and (4). Although there was a response in each case the effect of the stimuli extended over a much shorter period than in the demented patient. Beginning with the tenth kick preceding the point at which the stimulus was first given at (1), the length of the kicks in millimetres is given below.

5.5
5
5.5
5.5
3.5
10
4
4.5
4
15—(1)
8
13
4

2
3.5
4
14
9
5
10
6
3
5
2.5
15—(2)
3

2.5
9.5
2
2
2
5
7
15
4
15
4
5
12.5

10
7.5
3
3.5
3
3
7—(3)
4.5
3
15
3
4
2

7—(4)
14
5
8
2
7
6
10
7
9
6
8
6
11

In each of the four cases the two taps were given immediately preceding the kick designated by the figures (1), (2), (3) and (4).

It will be seen that the effect of the first stimulus can be traced over the three following kicks, and then the knee-jerk fell to 4 mm. The second stimulus caused a rise from 2.5 mm. to 15 mm., but at the next kick the knee-jerk fell to 3 mm. The third stimulus caused a rise from 3 mm. to 7 mm., and the next kick fell to 4.5 mm. As the effect of the stimuli was evidently diminishing, the fourth stimulus (4) was made much louder than the preceding ones, and the knee-jerk rose from 2 mm. to 7 mm., and then to 14 mm.; the succeeding three kick were 5 mm., 8 mm., and 2 mm.

In no instance did any such prolonged effect from the stimuli occur as is shown in the tracings from the demented patient. Fig. 12 also shows the "groups" of kicks that could not be identified with any external stimulus; these groups are well shown before the stimuli were given by the two taps. During the time that these groups appeared the subject was in the same condition of half-sleep as when the groups came out best in the demented patient.

It was possible on several occasions to find well marked evidences of this rhythm while the subjects were fully awake. Fig. 11 is a tracing from the demented patient while awake, and the wave like movement of the tops of the kicks is very evident.

As has already been stated there were suggestions of this rhythm (?) all through the tracings, from the first of the experiments, even during the waking state; but in none of the other tracings during the waking condition is it as well shown as in Fig. 11. This same suggestion of a rhythm is also shown in Fig. 13, which is of a tracing taken from a case of well advanced general paralysis. This patient was in the quiet and apathetic stage of the disease, much demented, with ataxic gait, and slow, stammering speech. He was awake during the whole experiment. The same wave-like appearance of the ends of the knee-jerks is well marked.

A tracing from still another patient is shown in Fig. 14. This man was a case of dementia, but not nearly so far advanced as the first case. He was awake during the experiment. There is seen the same suggestion of the wave-like motion of the ends of the knee-jerks.

A point not immediately connected with those already discussed, but having a bearing on the general question, is illustrated in Fig. 15, which is a portion of a tracing from the first case of dementia. The subject was fully asleep as is seen from the total disappearance of the knee-jerk during the first part of the tracing shown. At R the hammer fell out of time, interrupting the regular 5 seconds interval, and as a result of this disturbance of the regularity of the blows the knee-jerk rose during the next four kicks. At R^2 the rhythm of the blows was intentionally interrupted, with the result of causing a rise in the knee-jerk again, but this time less than at first, showing that the nervous system had become accustomed to this change of rhythm. At R^3 the blows were delivered in as quick succession as possible, causing a much greater increase of the knee-jerk than on either of the other two occasions; after eight blows given at the regular 5 seconds interval the knee-jerk again sank to 0.

Rhythmic Grouping.

Besides showing the peculiar prolongation of the effect of a sensory stimulus Fig. 1 shows also the peculiar "groups" of kicks that appear in the curves with no apparent auditory stimulus to account for them. One is tempted to speak of these groups as falling into a rhythm, but they do not occur under circumstances justifying one absolutely in making this claim. Yet looking at this curve as a whole it is difficult not to think that there must be some rhythmic periodical activity of the body to produce these wave-like rises and falls. It will be seen that there were four of these groups before the auditory stimulus was given at A and the blows at the beginning of the experiment appear to have begun at the top of one of these crests. After the effect of the stimulus given at A had disappeared two of these groups came on before the second stimulus was given at B, and after the effect of this had disappeared the groups continued to appear to the end of the experiment. It is to be noted in this connection that the reinforcement at A came at about the time when a "group" might have been looked for, and the question arises did the auditory stimulus simply intensify one of these periodical rises in the knee-jerk. At none of the places where these rises came on, except at A, B and C were there any reinforcing stimuli to account for them. At B also the stimulus appears to have come at about the time when a rise was due, while at C the stimulus seems to have come between two groups, and this may account for the fact that there was a shorter effect of the stimulus here than at A and B.

In examining the remaining diagrams showing the prolonged effect of sensory stimuli to see if one of these "groups" may have entered as a disturbing factor the only instance where this could be thought to do so is in Fig. 4 where the secondary rise after B may possibly be looked on as a "group," but the prolonged reinforcing effect of the stimulus at B is nevertheless sufficiently well marked. The "grouping" is again well shown in Fig. 8. The series is taken from the middle of a tracing after the patient was fully asleep. There was an interval of seven minutes between the first kick and the preceding one, and the first blows after this interval of rest naturally acted as a reinforcement, but the kicks soon diminished, and the peculiar wave-like movement of the curve developed. There were no sensory reinforcements from sounds and the subject seemed to be in the soundest sleep.

The same phenomenon is seen again in Fig. 9, where there were no sensory stimuli except at 1 where two taps were given, and at C where a passing train of cars was heard. This shows also that the two taps did not invariably call out the prolonged reinforcing effect that usually followed.

It was possible on some occasions to account for the changes by variations in the depth of sleep, as in Fig. 10. Here at the points marked A there were no auditory stimuli to account for the rises, but in the intervals, at the points marked B, there were audible evidences of the soundness of the sleep. Usually however it was not possible to detect such a change, and the waves rose and fell without any noticeable change in the regularity of the respiration.

The question as to the cause of this rhythmic (?) grouping of the knee-jerks in sleep is a very interesting one, and considerable attention was given to it in attempting to find an explanation. The phenomenon was noticed very early in the experiments, and many of the tracings made while the subject was awake suggest this same tendency to a periodicity. The "grouping" always shows best, however, in a condition that might be termed half-sleep, where the subject is to all appearances sound asleep, but that he is not in the profoundest sleep is shown by the fact that his knee-jerk is not entirely abolished. Attempts were made to find some connection between these "groups" and the respiratory rhythm, and the respiratory curve and the knee-jerk curve were taken simultaneously, but the results were entirely negative, no particular length of kick being found associated with a particular phase of the respiratory curve. The attempt was also made by taking a plethysmographic tracing from the arm, to find some connection between the depth of sleep and the variations in the knee-jerk, but these at first were equally unsuccessful. Later it was suggested that there might possibly be some connection between the groups and the Traube-Hering curves, and acting on this suggestion additional plethysmographic tracings were taken. A glass plethysmograph, suspended from the ceiling to allow free movement, was placed on the patient's left arm, and connection was made through a glass tube having rubber joints with a very sensitive Marey tambour, the writing-needle of which was placed directly over the writing-needle of the knee-jerk apparatus, so that the two curves were made synchronously on the revolving cylinder of the kymograph. The Traube-Hering curves did not always appear, and there were also many times when the peculiar "groupings" did not appear, as it was necessary for the patient to be in the condition of half-sleep already alluded to, and also that there should be few or no disturbing noises. The necessary conditions have been fulfilled on repeated occasions, however, and a series of tracings obtained where there is a good Traube-Hering curve and also a good series of "groups" of knee-jerks. A portion of one of these double curves is shown in Fig. 16. The pulse beats are well marked and the respiratory rhythm is also well shown. The jarring of the blow of the hammer was sufficient to set the needle of the Marey tambour violently oscillating, so that the respiratory rhythm appears to be unduly accentuated, but this sharp upward rise is due to the vibration of the needle. The Traube-Hering curve is also well marked, the tops and bottoms of the waves being connected by straight lines. It will be seen that there is an apparent coincidence between the two curves—that the Traube-Hering curve descends lowest in that part of the "group" where the kicks are longest, and at places where the Traube-Hering curve is highest the knee-jerks are much diminished. A rise in the Traube-Hering curve means of course increased blood pressure in the arm, and a fall in the curve corresponds to diminished blood pressure. On Mosso's theory that increased blood pressure in the extremities means lessened blood pressure in the central nervous system we should have, during the time that the Traube-Hering curve is at its height, relative anaemia of the brain and cord; and during the time that the Traube-Hering curve is lowest relative hyperaemia of the brain and cord. There are objections to Mosso's theory, however, as it fails to take into account the abdominal circulation, and the possibility that a change of blood pressure in the extremities may mean simply an opposite change in the abdominal cavity and not in the central nervous system.

Could we adopt Mosso's view it would simplify the problem greatly to say that if we got a rise in the Traube-Hering curve this would mean a diminished blood supply to the brain and cord, and a fall in the Traube-Hering curve would mean a corresponding increase of the blood supply of the central nervous system. The diminished knee-jerk would then naturally follow from the lessened functional activity of the spinal cord at the height of the peripheral Traube-Hering wave, while an increased knee-jerk from increased functional activity of the cord would follow at the low phase of the peripheral Traube-Hering curve. The occurrence of the high phase of the Traube-Hering curve with a diminished knee-jerk, and of the low phase with an increased knee-jerk has been noticed with sufficient frequency to give considerable probability to the theory that there may be a constant relation between the two. The Traube-Hering curves shown in Fig. 16 demonstrate that there is a rhythmic rise and fall in the blood pressure of the arm, as has been frequently proved before. There is thus naturally good reason to infer that with this rise and fall occurs throughout the whole vascular system, and that the vascular supply of the central nervous system is subject to this same periodicity. It also seems perfectly fair to assume that this rhythm might not necessarily be the same throughout the whole vascular system of the body. We only need assume that the vaso-motor centre in the medulla sets up the rhythmic contractions and dilatations in the vascular system that show themselves in the periphery in the Traub-eHering curves, but this rhythmic influence would not necessarily propagate itself throughout the whole body within the same time, the vascular system possessing considerable inertia, and the amount of blood to be influenced being so great. Several rhythms differing in time might easily be present in the vascular supply of different portions of the body, dependent on the different rates at which the vaso-motor influence had propagated itself through the arterial system; such rhythms should show the same general characters with regard to rise and fall. Fig. 17 shows the result of assuming that this rhythm in the central nervous system differs a little in time from the rhythm in the peripheral circulation. In Fig. 17 the Traube-Hering curve shown in Fig. 16 has been moved back a distance representing 20 seconds in time, which would mean that the vaso-motor influence affected the blood supply of the central nervous system 20 seconds before it reached the arteries of the periphery, which seems a not improbable supposition. Although even with this change, the crests of the Traube-Hering wave do not absolutely coincide with the points of the greatest diminution of the knee-jerk curve, and vice-versa, yet the coincidence is much more striking, and additional weight seems to be given to the inference that there may be some connection between the two curves. It will be noticed that at one point of the knee-jerk there was a disturbing factor caused by the slamming of a door below, at A, sending the knee-jerk up at this point, and thereby apparently making the summit of this knee-jerk wave farther along than it otherwise would have been.

In Fig. 18 there is a much closer coincidence in the two waves than in Fig. 16. Here the Traube-Hering curve makes a long descent between 4 and 5, with a still longer ascent to 6. Again it is to be noted that at 4, where the Traube-Hering wave descends lowest, the knee-jerk curve is also longest.

The same general coincidence of the two curves is again seen in Fig. 19. This was one of the earliest tracings, and the writing needle of the Marey tambour, connected with the plethysmograph, did not make as good a record as it did later. At the end of the record the patient fell into his deepest sleep with the entire disappearance of the knee-jerk. It is interesting to note that, although the Traube-Hering curve continues after the knee-jerk has entirely disappeared, yet at no point does the Traube-Hering wave descend as low as at A, where there was the longest group of kicks. As in Figs. 16 and 17, there is the same number of waves in the knee-jerk curve as in the Traube-Hering curve.

Fig. 20 is from another of the early tracings, but it serves to show the same general characteristics that have been brought out by the other curves. At A a secondary wave appears on the long descent between 6 and 7, and slight corresponding changes may be noted in the knee-jerks below.

All the plethysmographic tracings given were taken from the left arm. As it would be valuable confirmatory evidence to obtain similar tracings from the leg, a tin plethysmograph in the shape of a boot was made, and in this the patient's right foot and leg were placed; the rubber band that served to keep back the water coming just below the knee. (Into each plethysmograph water of 30° C. was poured to take up the extra air space not occupied by the leg and arm. The water did not quite fill the plethysmographs, a small air space being left at the top to allow free access of the air to the opening communicationg with the glass tube and Marey tambour. A much better tracing was made by the writing needle with the water in the plethysmograph than with this empty, as the pulsations were thus confined to the small body of air directly above the water, and the arc of vibration of the needle correspondingly increased). The leg plethysmograph was suspended from the ceiling in the same way as that for the arm, and it was still possible to have the left leg, which was still the upper one, in the same position as before for the knee-jerk experiments. Tracings were then made showing synchronous right leg and left arm plethysmographic curves and the left knee-jerk curve. It was found, however, that the jarring of the blow of the hammer on the left leg communicated itself through the bony pelvis sufficiently to affect the right leg in the plethysmograph, causing a serious vibration of the needle with each blow, interfering with the production of a good curve. The arm and leg plethysmographic curves were then taken without the knee-jerk curve with better results, and a general correspondence was found. The leg plethysmographic curve was never as satisfactory as that of the arm, for it was possible to place the whole arm in the plethysmograph, the strong pulsation of the radial artery being strong enough to give a well-marked tracing, but no similarly strong pulsation could be obtained from the foot and calf of the leg. While, therefore, the experiments with the leg plethysmograph need to be carried farther, yet so far they point to similar results as with the arm.

Should the conclusions suggested by the knee jerk and plethysmographic curves seem to be justified, and should they be borne out by further research, the knee-jerk would thus be brought into connection with the other rhythmical and periodic activities of the body. The vaso-motor influence that produces the Traube.Hering curves is necessarily constantly active, but its effects are usually obscured by many other conditions. It would appear probable on *a priori* grounds that the Traube-Hering curve would come out more clearly where the cerebral influence was removed or inhibited, and we find the Traube-Hering curve coming out with marked distinctness in our demented subject. For the same reason we should expect any phenomenon associated with the Traube-Hering curve also to come out better in such an individual than in a normal subject, and so we find the "groups"—if the relation to the Traube-Hering is a true one — coming out in this same patient. As the Traube-Hering curve is constantly influencing the normal respiratory rhythm, may we not also assume that the Traube-Hering knee-jerk curve, if it is permissible to call it such, is also constantly influencing the knee-jerk? This would explain the mysterious "rhythm" that has seemed to be present in many of the earliest knee-jerk curves taken in this patient, even when awake. We are led from this to a consideration of the knee-jerk of normal individuals, and if the inferences as to the influences affecting the knee-jerk in this demented man are legitimate, it is not evident why the same inferences do not apply to the normal subject. If this is the case then the original point of the investigation no longer has any bearing,—that is, the question as to the theoretically lesser variability of the knee-jerk in a demented person than in a healthy individual; the tops of the knee-jerks of a dement forming theoretically a straighter line than in a sound person. The "normal" knee-jerk curve, therefore, could no longer be considered as theoretically a straight line, but as a true curve correspondingin general with the Traube-Hering curve. It must be admitted at once that it is extremely doubtful if this can ever be shown on a normal individual with the constantly varying emotional condition of healthy persons; nor does it seem scarcely more likely that a normal individual will show such curves even in sleep as are seen in this patient, for the reason that the cerebral influences in a sound person would probably mask this ebb and blow; but this is mere conjecture and must be submitted to actual experiment.

It remains to add that if the "nervous force" or "irritability" of the spinal cord is really subject to this rhythmic action, the question is at once raised if the higher cerebral activities, especially the attention, are also subject to a similar rise and fall, for if the rhythm (?) already described be really due to a vascular process of vaso-motor origin, this same influence must affect also the functional activity of the brain itself.

Conclusions.

The chief interest in the results brought out in this paper lies in the fact that the experiments were conducted on a person whose mind has been weakened by dementia of many years

duration, and that this individual shows a greater susceptibility to sensory stimuli than persons in health. The most reasonable explanation of this seems to be that there has been a weakening of the inhibitory influence normally exerted by the brain over the lower centres. The remarks of Mitchell and Lewis[1] have so much bearing on this point that they may properly be quoted here. In discussing the cause of the increase of the knee-jerk from sensory stimuli, they say "It is very difficult to explain the fact that electricity, sensory impressions, and distant voluntary muscle acts increase the knee-jerk and the response to the muscle blow. If we conceive of a series of inhibitory centres extending from the mesocephalon all the way down the cord, and infer that all the agents mentioned are capable, by more or less paralyzing these centres, of releasing the active reflex groups below them, we shall be able to comprehend that the centre thus set free may, by increasing tone, give to the muscle a suddenly enlarged capacity to respond to the tendon taps or the muscle blow. Nearly all the facts with which we are concerned may be explained by inhibition organs and the effects produced upon them. On the other hand, it is equally conceivable that whenever a sensation reaches the cord or brain or both, an overflow occurs, which shall, by increasing the excitation of the centres, be felt throughout the body, and reinforce any organs chancing to be synchronously otherwise excited from without. Under this view we conceive of the nervous force as not confined entirely to the direct paths between the centres and the muscle to be moved, but as overflowing so as to pass through numerous ganglia, adding a certain small increment to their effects when in a state of such activity as the spinal toning centres must be at all times. The tone centres thus stimulated send out a higher wave of excitability to all the muscles, and if at the time this reaches a muscle, that muscle is being excited by a tap, there is an increased response."

Assuming such inhibitory centres as Mitchell and Lewis describe, these would only be kept up to their full functional activity by the healthy condition of the whole nervous system, so that when these inhibitory centres are under a weakened cerebral influence, as in dementia, they offer less resistance to what Mitchell and Lewis call the paralyzing influence of sensory stimuli. In the normal healthy individual in sleep a sensory stimulus preceding a blow on the patellar tendon produces a rise extending at most over a few kicks. This is certainly the most economical method for the individual, if we consider that it is beneficial that the effect of the accidental stimuli that are continually assaulting the nervous system should be prevented from spreading over a wide territory or through a long time. In our demented patient a stimulus produces an effect extending usually over a much longer time, even so long as three minutes.

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[1] Loc. cit. p. 203.

The experiments appear to give a graphic demonstration of the greater susceptibility to sounds, and all external stimuli, in persons with enfeebled, but not organically diseased, nervous systems. If this susceptibility to long continued effect of sensory stimuli in this patient is really due to weakened cerebral inhibitory power, it seems not improbable that the same effect must be produced in individuals with brains weakened or exhausted from any cause,—not necessarily from insanity. The large class of neurasthenic individuals naturally first occurs to one, and the question arises whether, when such patients "feel every sound in their back," this is not due to the uninhibited propagation of accidental external stimuli to the lower reflex centres, as shown in these knee-jerk curves. If so, the therapeutic corollary to this proposition, the necessity of excluding to the greatest possible extent all accidental stimuli from such patients, is graphically demonstrated in these curves.

The conclusions with regard to the so-called rhythm have already been discussed.

The results of the experiments may be summarized in the two following propositions:

In a case of terminal dementia of many years duration a series of experiments on the knee-jerk tend to show that:

1st. Sensory stimuli received during sleep produce a much greater effect and diffuse over a much longer interval than in healthy individuals.

2nd. In a condition of half-sleep when the patellar tendon is struck by blows of uniform strength at five seconds intervals, the knee-jerks fall into groups, and synchronous plethysmographic tracings suggest that these groups have some connection with the Traube-Hering curve.

If the truth of the second proposition can be conclusively established, several important corollaries would seem to follow. These are here stated as facts for the sake of presenting definite propositions, the truth or falsity of which must be submitted to further experimental investigation.

(a) The knee-jerk curve, instead of being theoretically a straight line as has been heretofore assumed, is, in reality, a curved line, with the general characteristics of the Traube-Hering curve.

(b) The spinal cord is not constantly in a condition of the highest potential functional activity, but its activity is represented by a curve of rhythmic vascular contraction and dilatation. During the phase of contraction of the spinal arteries, the spinal cord is at its least functional activity, due to a condition of relative anæmia, while during the phase of dilatation of the spinal arteries, the spinal cord is at its greatest functional activity, due to a condition of relative hyperæmia.

(c) The question inevitably raised by (b) is whether the higher activities of the brain are also subject to a rhythmic rise and fall synchronous with vascular dilatation and contraction.

It remains to express my obligations to several who have assisted me in the details of the work, and to extend my thanks to my colleague, Dr. D. H. Fuller, and to Drs. Abbot, Young, Fitz and Sawyer, medical house-pupils, for their assistance. To Dr., Fitz I am especially indebted for assistance and suggestions in regard to apparatus.

THE GROWTH OF MEMORY IN SCHOOL CHILDREN.

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During the Spring of 1891, by permission of the School Board in Worcester, Mass., Dr. Franz Boas of Clark University took certain anthropological measurements of the pupils in the Grammar Schools, and also made certain tests of eye-sight, hearing and memory. The memory tests, which were made upon about fifteen hundred pupils in the Grammar Schools, above the second grade and below the high school, together with some tests from the Normal School, came into my hands for examination. To complete the material for all the grades in the public schools, the tests were made upon some of the senior and sophomore pupils in the High School.[1]

The Method of Making the Tests.—A series of numbers in which the digits were so arranged that they did not stand in their accustomed order and no digit was repeated, was read before each class to be tested, and each class was tested on four different occasions. In two Grammar Schools and in the Normal School, where the purpose was to determine the effect of fatigue, two of the tests were taken in the morning immediately after school assembled and the other two just before closing in the afternoon. In the other schools the tests were made in the morning. The digits were dictated slowly and distinctly at intervals of about two-fifths of a second with care to avoid rhythm or grouping, and at a given signal after the dictation of each number was finished, the pupils wrote the digits as they remembered them. To avoid a confusion of terms, *observation* will be used to indicate a group of five or more digits ; *digits*, to designate the figures ; and *place*, to indicate the position or order from the beginning of the number—observation. In the lower grades of the Grammar Schools and in the High School twelve observations constituted a test, but in the eighth and ninth grades only nine observations were made at each test. In the lower grades the first three observations of each test were made with five digits, the second three with six; the third three with seven; and the last three with eight. In the higher grades and in the High School the first three observations were made with six digits, and in the Normal School with seven. The tests being repeated at four different times, twelve observations with five, six, seven and eight digits respectively, were made upon each pupil, and it would be possible for each pupil to get any number of the twelve correct. An observation was considered correct when only those figures which the teachers had dictated were present in the same order as that in which they had been dictated. The various kinds of errors will be treated further on. When the observations at each test are begun with five-place numbers and gradually increased to eight, the pupils easily grasp the five-place numbers and are led by these to grasp and retain more than they would otherwise be able to do. If the observations are begun with seven-place numbers and only one observation made with the seven-place number before making one with an eight or a nine, the number of figures is not so easily remembered, and more errors result from this cause. When we come to discuss the various classes of errors in the observations on the Normal School pupils, this matter will come up again. In the sixth grade of one Grammar School through a misunderstanding on the part of the teacher, all the observations taken at the first test were made with five-place numbers, those at the second with six, at the third with seven, and at the fourth with eight. After two or three trials the pupils became aware of the number of digits to expect at each test and gave their attention more to retaining the proper digits in their places. As the number of pupils is so small and the results from this grade do not differ more from the results of the next higher or lower grade than the results of some other grades differ from those of the next higher or lower grade, these pupils have been included in the charts showing the curves for ages and grades.

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[1] I wish to acknowledge my indebtedness to Dr. Boas for this material and his advice in regard to the method of treating it. At his suggestion I have introduced the theoretical treatment of the curves. I am also indebted to Dr. E. C. Sanford for helpful suggestions, and to the teachers of the public schools who have assisted in collecting this material.

By classifying the pupils of a particular age or in each grade according to the proportion of the twelve observations on five-place numbers that were correct, and the same for six, seven and eight-place numbers, and marking the percentage that each class was of the total number of pupils in the grade or of the age on thirteen ordinates (twelve for the twelve observations and one for those pupils who had none correct), and connecting these points by a line, a curve representing the distribution of the pupils of the age or grade will be obtained. The maximum of the curve will then show the proficiency of the pupils for the age or grade in remembering five, six, seven or eight-place numbers, as the case may be.

Upon Chart I the curves show the distribution of the pupils according to the ages and upon Chart II the distribution according to grades. As the observations were made with five, six, seven and eight digits, four sets of curves will be found upon each chart. The curves for the grades in which eight-place numbers were used, are found at the top of the chart, and those for seven, six and five-place numbers in order below. Under each curve appear the number of pupils and their ages on Chart I and their grades on Chart II. To give a graphic representation of the value of these curves, which is to show the increasing accuracy with which the older pupils remember a given number of digits, the average percentages of pupils in each grade and of each age, who have got six or more (for five-place numbers, ten or more) of the twelve observations correct have been taken and this average has been marked upon the twelfth ordinate of each curve. When these points are connected in each set of curves a line is obtained, the rise in which, from left to right, will then represent the increasing accuracy with which the older pupils and the pupils in the higher grades remember a given number of digits. This line, of course, is arbitrary, but it will be found to correspond very closely with the probabilities of errors for the curves. A comparison of these lines will show a more uniform rise in Chart I Chart II shows that the eighth and ninth grades fall below the seventh on eight-place numbers; on seventh-place numbers, the fifth grade falls below the fourth, the eighth below the seventh, and the High School below the ninth grade. Other cases need not be mentioned. On Chart I only two actual falls are noticed, and these are less than one per cent. Considering that our tests measures the length of the memory-span, we can conclude that the memory-span increases with the age rather than with the growth of intelligence, as determined by the tests used in promoting pupils from one grade to another. Our tests do not apply to the retentiveness of the memory. They may be considered as tests of the power of concentrated and sustained attention. My own experience and observations upon the pupils while the tests were being made, seem to indicate that most pupils depend upon their powers of visualization to remember the number of digits, and at the same time they were noticed to repeat the digits as they were dictated. A comparison of the standing of pupils in their grades and their ability to remember figures was undertaken to determine, what was the relation between the memory-span and intellectual acuteness of the pupils. As the pupils depend upon their powers of visualization, this subject becomes more interesting in determining how far this power is of service in school work and how closely the power of concentrated and sustained attention is related to intellectual acuteness. For this purpose the teachers in the Oxford and Freeland Street Schools and of the High School were requested to give an estimation of what they considered was the general standing of their pupils with respect to the school work. The pupils were classed as either good, fair or poor, and these classes were compared with three classes which were determined in the memory test by the proportion of correct judgments. The percentages of pupils, for whom the two methods of ranking gave the same results, are given in the table. The letters of the following table (A representing good, B fair and C poor) in the left hand column indicate the teachers' classification, and those in the upper line the classification by the memory test.

TABLE I.—Giving the comparison of the teachers' estimation of standing of the pupils, and their standing, as determined by the memory tests. The classes of the teachers are represented as 100 and the others are expressed in percentages.

	A	B	C
A	32.6 %	51. %	16.3 %
B	21.4 %	58.2 %	20.4 %
C	24.1 %	49.4 %	26.5 %

Only eight and a half per cent. more of the pupils who were classed A by the teacher have been classed A rather than C by the memory tests. If our tests may be considered as tests of the ability for concentrated and sustained attention and of the power of visualization, we can conclude that these powers are not the only ones concerned in intellectual pursuits and are not sufficient for a successful undertaking of intellectual work. Intellectual acuteness, while more often connected with good powers of visualization and of concentrated attention, does not necessarily require them, and it cannot be said that those pupils who are bright intellectually are more distinguished on account of their good memories. The fact that a good memory is not necessarily accompanied by intellectual acuteness, adds weight to the conclusion that the growth of the memory does not necessarily accompany intellectual advancement.

Theoretical Treatment of the Curves.—If we consider that the twelve observations made upon each pupil are subject to the law of chance, then we can construct from the probability of error for any curve the corresponding theoretical curve, and if our supposition is correct, the theoretical and actual curves should correspond very closely. In saying that the answers of the pupils are subject to chance we mean that they are just as likely to err on one observation as upon another, and if this is true, we can treat these observations according to the law of probability. The probability of error for any curve is obtained by subtracting the actual number of correct judgments from the possible number and finding what percentage this difference is of the possible number of correct judgments. With this percentage as the probability of error, we construct the theoretical curves according to the formula

$$p^{12}+\tfrac{12}{1}(1-p)p^{11}+\tfrac{12}{1}\cdot\tfrac{11}{2}(1-p)^{2}p^{11}+\tfrac{12}{1}\cdot\tfrac{11}{2}\cdot\tfrac{10}{3}(1-p)^{3}p^{10}+ \\ \ldots\ldots+(1-p)^{12}$$

in which p represents the probability of error. When we construct these theoretical curves, which are found in Table II, we find they do not agree except in a few cases with the actual curves. The form, however, is somewhat the same, but the absolute values are different. Either the supposition is not correct and hence this treatment is not possible, or some element has entered in this case which prevents this material from being so treated. The latter alternative is, perhaps, the correct one.

With completely naïve subjects and like conditions it does not seem probable that twelve observations upon one pupil would differ from one observation upon each of twelve pupils; and yet the variations in the probabilities for the different pupils, when classed according to age or grade, is so great that we might not get an exact correspondence between the theoretical and actual curves even with the many observations under the most uniform conditions. As we shall show further on that the children increased in their power to remember figures with each succeeding test and in one school they were not completely naïve with respect to the tests, a possible explanation is found for this disparity between the theoretical and actual curves. This explanation is further strengthened by the fact that in those curves, where the probablity of error is less than five per cent., there is a close correspondence between the theoretical and actual curves (see Table II.). Where the probability of error is less than five per cent. for the first test, little increase in the accuracy of the judgments took place for the succeeding tests and hence the law will apply.

The actual curves are compounded of the curves represented by the probabilities of each pupil of the age or grade; the percentages of increase with each test show that there was a different probability for each observation. The effect of compounding a curve of several curves with very different probabilities is to broaden and flatten it, and it is just in this respect that the actual curves differ from the theoretical. If we construct the theoretical curves from the probabilities for the four tests on seven-place numbers given in Table VII. and compound these, we get a curve very much flatter than any of the theoretical curves. The absolute values of this curve are 0.2, 1.4, 4.7, 10., 15.3, 18.1, 17.8, 14.5, 9.8, 5.4, 2., 0.5 and 0. The probability for each test represents a curve compounded of three other curves which would have the tendancy to modify further in the same way the curves of which we have given the absolute values. This probably explains the disparity in form between the theoretical and actual curves and in view of the number of pupils represented by each curve the individual probabilities may account for the irregularities.

In Table II. the percentages for the theoretical and actual curves for the three Grammar Schools are given. The first part of the table is taken up with the actual curves, and the second with the theoretical curves. At the top of the table the numbers of the ordinates and the probabilities of error corresponding to each are given. In the columns, below the percentages of pupils for the different ages and grades and for five, six, seven and eight-place numbers are given under each ordinate for the actual curves. In the last column the figures represent the probabilities of error for the curves. For the theoretical curves these values are assumed. To make a comparison of the theoretical and actual curves take the probable error for any actual curve and find the theoretical curve whose probability of error most nearly corresponds to it. If the two curves correspond, the absolute values should agree closely.

TABLE II.—Giving the percentages of pupils and probabilities of error for the theoretical curves and for the actual curves of both the grades and the ages.

PART 1. *Actual Curves for Ages on 5, 6, 7 and 8 Digits.*

Number of Ordinate.		0	1	2	3	4	5	6	7	8	9	10	11	12	Probabilities of Error.
Probabilities of Error for each Ordinate.		% 100	% 91.6	% 83.3	% 75	% 66.6	% 58.3	% 50	% 41.6	% 33.3	% 25	% 16.6	% 8.3	% 0	
No. Digits.	Age.														
5	8 yrs.	0	0	1.	2.5	2.5	1.	4.	6.5	8.	12.	12.	14.5	34.5	20.
	9 “	0	0	2.	2.	0	1.	4.5	2.	4.5	7.5	10.	20.	45.5	14.5
	10 “	0	0	0	1.	1.	2.	1.	2.	6.5	8.5	13.	20.5	45.	12.
	11 “	0	0	0	0	0	0	1.	1.	5.	5.	10.	22.5	53.5	9.3
	12 “	0	0	0	0	0	0	3.	3.	3.	5.	8.5	18.	60.0	7.4
	13 “	0	0	0	0	0	1.	0	1.	1.	1.	17.5	18.5	58.5	6.6
	14 “	0	0	0	0	0	0	0	0	0	4.5	12.	17.	67.5	4.6
6	8 “	6.5	8.	13.	9.	9.	9.	10.5	6.5	6.5	2.5	5.	5.	8.	56.3
	9 “	7.5	6.5	9.5	6.5	9.5	5.5	3.	6.5	7.5	8.5	7.5	14.	9.5	45.5
	10 “	5.5	3.	6.5	5.5	9.5	9.5	6.5	3.	11.	9.5	9.5	15.	5.5	42.5
	11 “	1.	1.	4.	4.	5.	6.	12.	5.	13.	18.	9.5	8.5	15.	32.4
	12 “	1.	2.5	4.	2.5	4.	7.5	8.5	7.	11.5	14.5	8.5	15.	15.	31.3
	13 “	1.5	0	4.5	1.	5.5	2.5	7.	9.5	4.5	9.5	9.5	19.5	25.	27.7
	14 “	0	0	1.	1.	7.	3.	3.	12.5	10.5	11.5	14.5	11.5	25.5	23.5
	15 “	1.	1.	1.	2.5	2.5	3.5	4.5	12.	9.	11.	8.5	19.5	23.	25.3
7	8 “	40.5	16.5	14.5	2.5	0	2.	2.	2.	0	5.	0	5.	5.	78.
	9 “	15.5	8.5	8.5	14.	7.	9.5	7.	7.	8.5	4.	4.	4.	2.5	64.6
	10 “	19.5	13.	10.	8.5	9.	5.5	9.	8.5	3.5	5.5	2.	5.5	1.	66.2
	11 “	11.5	9.5	8.5	8.5	5.	9.5	11.5	7.5	6.5	6.5	6.	4.	4.	62.5
	12 “	12.5	5.	6.5	9.	5.	5.5	8.	12.5	7.5	9.	10.5	5.5	4.5	51.5
	13 “	5.5	5.5	4.5	8.	5.5	13.	8.	7.	5.5	7.	16.5	11.	3.5	49.2
	14 “	2.	5.	8.5	8.5	6.5	9.5	5.	5.	8.5	13.5	11.5	8.5	8.5	36.1
	15 “	9.5	6.5	7.5	6.5	6.5	1.	8.5	6.5	10.5	10.5	9.5	12.5	10.5	40.7
8	11 “	56.	15.5	8.	10.	2.5	2.5	5.5	1.	3.5	2.5	0	1.	2.5	84.
	12 “	23.5	15.5	12.5	5.5	6.5	4.5	8.5	4.5	3.5	4.5	2.5	2.	3.5	73.7
	13 “	25.5	17.	10.5	8.5	5.5	5.5	5.5	4.5	5.5	1.	6.5	4.5	1.	70.5
	14 “	22.5	8.5	12.5	13.5	4.5	5.5	5.5	5.5	3.	5.5	3.	7.5	3.	65.5
	15 “	18.5	12.5	7.	14.5	8.	6.5	7.	11.	4.5	6.5	2.	0	2.5	66.2

Actual Curves for Grades on 5, 6, 7 and 8 Digits.

No. Digits.	Grades.														
5	3	0	1.	1.5	3.5	3.5	1.	3.5	5.5	10.	12.5	17.5	19.5	23.	21.3
	4	0	0	0	0	0	0	1.	2.5	4.5	6.	11.	19.	57.	6.1
	5	0	0	0	0	0	1.	1.5	1.5	4.5	5.5	16.	24.	46.	8.4
	6	0	0	0	0	0	1.	0	1.5	3.	4.5	11.5	12.5	66.	6.5
	7	0	0	0	0	0	0	1.	0	0	1.	6.	15.	77.	1.6
6	3	11.	9.5	13.5	10.5	12.5	11.5	7.5	4.5	6.	2.5	3.5	3.5	3.5	63.
	4	3.5	4.	4.5	4.5	5.	7.5	5.5	5.5	7.	13.	14.	15.5	10.5	31.9
	5	1.5	4.5	7.5	5.5	9.5	8.	9.5	8.5	12.	11.	8.	11.	5.	43.
	6	1.5	1.	2.5	1.	6.5	7.5	10.5	10.5	10.5	17.5	8.5	11.5	12.5	32.
	7	0	1.	2.	3.5	2.	2.	3.5	4.5	4.5	9.	13.	23.	32.5	16.7
	8	.5	.5	2.5	0	3.	3.	6.	13.	8.5	14.5	13.	15.	20.5	16.6
	9	0	0	1.5	2.5	4.5	4.5	3.	7.	10.5	5.	8.5	22.5	32.	16.5
	H.S.	0	0	0	0	2.	2.	2.	0	6.	6.	8.	30.	46.	9.

Actual Curves for Grades on 5, 6, 7 and 8 Digits.—Continued.

Number of Order.		0	1	2	3	4	5	6	7	8	9	10	11	12	Probable Errors.
Probable Error of each Order.		% 100	% 91.6	% 83.3	% 75	% 66.6	% 58.3	% 50	% 41.6	% 33.3	% 25	% 16.6	% 8.3	% 0	
7	3	46.	18.	15.	4.5	3.	2.	4.5	2.	0	1.5	1.5	0	0	87.7
	4	19.	10.5	11.	8.	4.5	5.5	11.	5.	4.	8.	3.	7.	2.	63.3
	5	19.	11.	6.5	12.	7.	12.	9.5	11.	1.5	1.5	3.5	2.5	1.5	67.
	6	7.5	9.5	7.5	11.5	7.5	6.5	10.5	10.5	6.5	7.5	9.	3.5	2.5	59.
	7	6.	2.	6.	6.	5.	5.5	6.	8.5	9.5	11.	15.5	13.5	5.5	46.4
	8	6.	6.	4.5	8.5	6.	8.5	8.5	8.5	11.	9.	9.	9.	7.5	44.5
	9	3.5	4.5	5.5	3.5	3.5	2.5	4.5	5.5	7.5	13.	15.5	15.5	15.5	32.5
	H.S.	2.	4.	8.	10.	10.	10.	6.	4.	10.	12.	12.	10.	2.	50.
8	6	37.5	19.	11.	7.5	6.5	5.5	.5	1.5	2.5	1.	.5	1.5	0	85.
	7	11.	9.	12.5	13.5	6.5	5.5	8.5	6.	6.	6.	7.	6.	4.5	57.5
	8	22.5	16.	11.	12.5	6.	4.	5.5	7.	5.5	2.5	4.	1.5	2.5	70.
	9	19.	10.	8.5	12.5	5.5	7.	6.	12.	2.5	9.	1.5	2.5	3.	64.8
	H.S.	18.	18.	6.	12.	6.	10.	10.	6.	4.	4.	4.	2.	2.	67.

PART 2.—*Theoretical Curves.*

		0	0	0	0	0	0	0	0	0	.3	3.2	22.7	73.8	2.5
		0	0	0	0	0	0	0	0	.3	1.7	9.9	34.1	54.	5.
		0	0	0	0	0	0	0	.7	1.4	4.6	17.	38.1	39.3	7.5
		0	0	0	0	0	0	0	1.5	2.1	8.5	23.	37.6	28.2	10.
		0	0	0	0	0	0	.4	1.7	6.8	17.2	29.2	30.1	14.2	15.
		0	0	0	0	0	.4	1.5	5.3	13.3	23.6	28.3	20.6	6.8	20.
		0	0	0	0	0	1.1	4.	10.3	19.1	25.8	23.3	12.7	3.1	25.
		0	0	0	.1	.7	2.9	7.9	15.8	23.1	24.	16.8	7.1	1.3	30.
		0	0	0	.4	1.9	5.9	12.7	20.4	23.7	19.5	10.9	3.6	.5	35.
		0	0	.2	1.2	4.2	10.1	17.7	22.7	21.3	14.2	6.3	1.7	.2	40.
		0	.1	.6	4.3	7.2	14.9	21.3	22.3	17.	9.2	3.4	.7	.07	45.
		.02	.3	1.6	5.3	12.1	19.4	22.6	19.4	12.1	5.3	1.6	.3	.02	50.
		.07	.7	3.4	9.2	17.	22.3	21.3	14.9	7.2	4.3	.6	.1	0	55.
		.2	1.7	6.3	14.2	21.3	22.7	17.7	10.1	4.2	1.2	.2	0	0	60.
		.5	3.6	10.9	19.5	23.7	20.4	12.7	5.9	1.9	.4	0	0	0	65.
		1.3	7.1	16.8	24.	23.1	15.8	7.9	2.9	.7	.1	0	0	0	70.
		3.1	12.7	23.3	25.8	19.1	10.3	4.	1.1	0	0	0	0	0	75.
		6.8	20.6	28.3	23.6	13.3	5.3	1.5	.4	0	0	0	0	0	80.
		14.2	30.1	29.2	17.2	6.8	1.7	.4	0	0	0	0	0	0	85.
		28.2	37.6	23.	8.5	2.1	1.5	0	0	0	0	0	0	0	90.
		39.3	38.1	17.	4.6	1.4	.7	0	0	0	0	0	0	0	92.5
		54.	34.1	9.9	1.7	.3	0	0	0	0	0	0	0	0	95.
		73.8	22.7	3.2	.3	0	0	0	0	0	0	0	0	0	97.5

In Table III are given the probabilities of error for every curve upon Charts I and II. Part I of the table is taken up with the curves for ages and Part II with the curves for grades. These probable errors show the same general results that the line drawn across the curves shows. When the pupils are classified according to their ages, the figures representing the probable errors show a more uniform decrease in passing from the younger to the older pupils than from the lower to the higher grades. The High School pupils[1] are not included in the classification for ages. Where the probabilities of error for the higher grade is greater than for a lower, or for older than for younger pupils, the number has been set in heavy faced type.

TABLE III.—Probabilities of Error upon 5, 6, 7 and 8 digit series for all ages and grades.

PART 1.—*Probabilities of Error for Ages.*

No. Digits	8 yrs.	9 yrs.	10 yrs.	11 yrs.	12 yrs.	13 yrs.	14 yrs.	15 yrs.
5	20.	14.5	12.	9.3	7.4	6.6	4.6	
6	56.3	45.5	42.5	32.4	31.3	27.7	23.5	**25.3**
7	78.	**64.6**	66.2	62.5	51.5	49.2	36.1	**40.7**
8				84.	73.7	70.5	65.5	**66.2**

PART II.—*Probable Errors for Grades.*

No. Digits	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	H. S.
5	21.3	6.1	**8.4**	**6.5**	1.6			
6	63.	31.9	**43.**	**32.**	16.7	16.6	16.5	9.
7	87.7	63.3	**67.**	59.	46.4	44.5	32.5	**50.**
8				85.	57.5	**70.**	**64.8**	**67.**

In psycho-physical experiments it is customary to take seventy-five per cent. of right answers as the point at which the subject may be safely said to have some knowledge of that concerning which he judges. This standard is chosen for experiments in which a choice is made between two alternatives, where, by mere guesses, the subject will get 50 per cent. correct. In our test the subject must be supposed to have exact knowledge before he can recall correctly any number of digits. Whatever standard we choose, then, for these tests, it must be considered as the probability that a certain number of digits should be judged correctly every time. If we choose seventy-five—though it seems to me a less figure might be chosen—our tables show that all the pupils below the 6th grade and over thirteen years of age reach the limit of their memory span at six, and all others at seven. Six may then be taken as the limit to the memory span for most Grammar and High School pupils.

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[1] The Normal School pupils have been purposely left out of this part of the treatment. The tests were not made with sufficient uniformity to allow them to be classed with the public school pupils.

Any treatment of a subject of this kind would be incomplete if no comparison were made between the boys and the girls. For this purpose the boys and girls have been classified according to their ages; in order to get classes sufficiently large to form a comparison, it was necessary to put the pupils differing by two years in age instead of one into each class. The probabilities of error have been found for each class and the comparison is made in the following table. The ages together with the probabilities of correct judgments are given for each class.

TABLE IV.—Showing separately the probability of Error for Boys and Girls.

FIVE-PLACE NUMBERS.	Boys.	Girls.
Pupils under 10 years	16.	13.7
Pupils over 10 and under 12	10.4	11.5
Pupils over 12	11.5	11.6
SIX-PLACE NUMBERS.		
Pupils under 11 years	47.6	47.9
Pupils over 11 and under 13	35.2	26.5
Pupils over 13	25.4	25.
SEVEN-PLACE NUMBERS.		
Pupils under 12 years	65.6	61.6
Pupils over 12 and under 14	50.7	51.
Pupils over 14	51.	44.
EIGHT-PLACE NUMBERS.		
Pupils under 14 years	82.4	64.7
Pupils over 14 years	65.4	65.8

From this table it will be seen that in a majority of classes the girls make a decidedly less error than the boys. In the classes where the boys surpass the girls, it is by a very small figure. This conclusion harmonizes with the results of other observers.[1]

Unconscious Memory and Effect of Fatigue.—The tests were taken in three different Grammar Schools: Oxford, Freeland and Woodland Street Schools. In the Oxford Street School the four tests were taken in the morning and a different series of digit-groups were used at each test. The same digits dictated at the first test were read in the inverse order at the second. They were then completely re-arranged for the third and read in the inverse order for the fourth. Thus the digits in every observation were the same for the four tests, the order alone being changed. This same arrangement was used in the tests of the Freeland Street School, two tests being taken in the morning immediately after the school assembled, and two just before closing in the afternoon. In the Woodland Street School the same digit-groups were used for all four tests, the purpose being to determine the effect of unconscious memory.

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[1] A Statistical Study of Memory and Association, by Prof. Joseph Jastrow, Educational Review, Dec., 1891.

In the following Table the probabilities of correct judgments for each test on five, six, seven and eight-place numbers for all the pupils in Oxford Street School are given.

TABLE V.—Shows the probabilities of correct judgments in the Oxford Street School for the four tests with five, six, seven and eight-place numbers; 136 pupils were tested in this school.

No. Digits.	First Test.	Second Test.	Third Test.	Fourth Test.
	A. M.	A. M.	A. M.	A. M.
5	82.7	91.3	83.	91.3
6	53.1	73.	71.	73.5
7	30.4	30.1	33.5	39.2
8	17.6	16.3	19.4	25.

In this school, where different series of digit-groups were used at each test, the pupils show with two exceptions considerable though not uniform increase in their ability to remember the groups of digits. This increase may be fairly taken to be the effect of practice, as the pupils remained naïve as far as possible with respect of the tests that were to be used.

TABLE VI.—Shows the probabilities of correct judgments for the Freeland Street School on four, five, six, seven and eight-place numbers. The digit groups that were used in the Oxford Street School were used in this school. Two tests were made in the morning and two in the afternoon; 219 pupils were tested in this school.

No. Digits.	First Test. P. M.	Second Test. A. M.	Third Test. P. M.	Fourth Test. A. M.
4	92.	95.	98.	95.
5	79.3	86.7	95.9	79.5
6	60.1	65.6	64.7	60.2
7	37.9	43.2	43.3	44.6
8	25.6	25.7	32.7	32.2

This Table shows that the pupils improved considerably though not uniformly with each test. They do not show greater increases for the morning than for the afternoon tests as we should expect from the fatigue of the day's work.

TABLE VII.—Showing the probabilities of correct judgments for the Woodland Street School on four, five, six, seven and eight-place numbers. The same series of digit-groups were used in all four tests. Two tests were made in the morning and two in the afternoon; 468 pupils were tested in this school.

No. Digits.	First Test. A. M.	Second Test. P. M.	Third Test. A. M.	Fourth Test. P. M.
4	96.2	97.3	98.	97.7
5	88.6	92.	94.2	94.3
6	56.7	64.4	70.1	75.5
7	40.4	50.7	58.7	64.1
8	28.4	34.9	45.9	49.7

In this school the pupils have shown uniform improvement in each test and at the same time the percentages of increase are usually larger. The morning tests do not show greater proportional increases than the afternoon.

The results from all the schools point to the conclusion that the pupils improve with practice. The great uniformity and large increases with each test in the Woodland Street School seem to show that the pupils unconsciously remember the digits that have been dictated one day previous. The probabilities of correct judgment do not show any variations due to fatigue. The total number of correct judgments for the morning tests in the Freeland Street School are 2,69 and for afternoon tests 2,640; for the morning tests in Woodland Street School 6,609, and for the afternoon tests 7,179. When we consider that great increases were made with each test, and the first test in the Freeland Street School was made in the afternoon, we should expect a greater number of correct judgments for the morning test; and since the first test was made in the Woodland Street School in the morning, we should expect a greater number of correct judgments for the afternoon test. This is just what the figures show, and we may safely conclude that the pupils suffer no fatigue from their school work, at least none discoverable by such tests as these. Their work is probably not excessive.

The Nature of Errors.—A careful examination of the observations shows that there were three, perhaps four, classes of errors which represent stages in the fading of the memory-image. In the first stage the digits suffer a displacement of order; in the second, other digits are substituted for some that were dictated and in the third, the number of digits is misjudged, either over- or under-estimated. Various causes may be assigned for the displacement of order. When the pupil attempts to write, the attention passes over the successive digits in memory as a rule much faster than they can be written. Before the pupils can write the first digit, the attention has passed to the third or fourth and the hand is innervated for the digit that is present in consciousness. The second may be immediately recalled and is put in the third place. It more frequently happens that the fourth or fifth is displaced than the second or third. Again, the order of the digits in the numbers previously dictated clings in the mind and causes the figures in the next number to be interchanged in accordance with that order.[1] A single case will be sufficient to make this statement clear. Two numbers, the first commencing with 8163 and the second with 5136, were dictated. The 3 and 6 in the second were frequently reversed so as to read 63. Further, the order in which the digits stand in our system of notation determines some changes. One case in particular deserves mention and will be of service to others who attempt any work in this line. The last three digits of one number were 768 and a very frequent error was to change the order of the 7 and 6 to 67. It is probable also that when these digits were read in the inverse order, 867, the order of the 6 and 7 is again changed to read 876. In other cases a digit seems to have become associated with one place in the number from having frequently occupied that place, and when this digit appears in the next succeeding number, it changes places with the one that occupies the position it has become associated with; even when it does not appear in the following number, it may be substituted for one that occupies its associated position. In many cases it is very difficult to determine what has brought about the change and whether the error is an error of inversion or substitution. Number habits and the association of one digit with another from some experience in life—the number of the house, the year or day of the month of a child's birth—would seem to enter as factors. A fact that has been noticed frequently in teaching children and also adults is the great liability to confusion, when it is attempted to keep separate two like organs whose functions are diametrically opposed. In physiology it is a difficult matter for children to distinguish the functions of the right and left ventricles of the heart and even for adults the functions of the dorsal and ventral columns of the spinal cord. It seems probable that this difficulty also appears in keeping the order of two digits that are easily remembered. 1 and 5 standing at the end of a number, where the digits are rarely forgotten were frequently interchanged. The inversion of the order is by far the most frequent error, as it is also the first to occur.

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[1] Dr. Leo Bergerstein, Zeitsch. f. Schulgesundheitspflege, No. IX and X. 1891.

In substituting a new digit for one that has been read, there enter some of the causes that bring about an inversion of the order. A digit is substituted for another to make the two stand in the order they do in our system of notation, or in the order in which they were in the number previously dictated. The likeness in the sounds of the names of two digits often determines the substitution in the one for the other. Nine and five and nine and one are frequently interchanged. The written or printed forms of 9 and 1 probably have something to do with the substitution of the one for the other. The very frequent interchange of 3 for 8 is due unquestionably to the likeness in the form of the printed digits. The likeness in the innervation required for two digits would seem to explain the substitution of 5 for 3 and 7 for 9. Substitution stands next in frequency to inversion of the order.

When the digits are left out the pupils more frequently have forgotten the proper digits and also their associations and so drop them out altogether. Whatever may be the cause of the dropping of a digit, the fact that it is left out shows a more advanced stage in the disappearance of the memory-image. The places in which the most errors of every kind are likely to occur are the positions from which digits are most frequently dropped. In some cases it seems probable that a digit may be dropped from the tendency to bring two associated digits in juxtaposition or two digits that stand juxtaposed either in our system of notation or in some number previously dictated.

When the pupils overestimate the number of digits, two tendencies only were noticed. The digits that were supplied were put in the places in our system of notation that occur between some two digits already given, or they were placed between two digits which should stand together and which were separated by the supplied digits in some number previously dictated. When two digits already stand in their natural order, the tendency is very strong to put another digit in order either before or after those given. The second tendency was to repeat some digit already written. Over-estimations of the number are very infrequent, probably for the reason that each test was begun with numbers that could be easily grasped and digits that could be counted. In the Normal School the observations were begun with seven-place numbers; but instead of making three observations, as was done in the Grammar Schools, with seven digits, the teacher dictated only one seven-place number before dictating an eight- and a nine-place number. Again, a seven- and an eight- and a nine-place number were dictated and so on until fifteen observations were made at each test. As the curves have shown that six figures are all that the best pupils can easily span, the Normal School pupils were taxed to the limit of their powers on the first trial. In the Grammar Schools the pupils were started with numbers they could easily grasp and were led by steps to expect the number of digits in each succeeding observation. On this account 180 pupils from the Oxford Street School over-estimated the number of digits on observations with seven- and eight-place numbers 88 times; and 24 Normal School pupils over-estimated the number of digits 76 times. In counting the errors that arise from dropping digits no separate account was taken of the cases where the pupils dropped the digits because they did not remember the number of digits given, and where they dropped them because they failed to recall the correct digits. In most cases it would be difficult to determine this. This tendency, however, to over-estimate shown by the Normal School pupils is not the general rule, as the experiments of other observers have pretty conclusively shown. Drs. Hall and Jastrow[1] found that the tendency was to under-estimate the number of clicks made by a quill held against the notched circumference of a revolving wheel, when the number of clicks was too rapid to be counted. Other experiments point in the same direction. It would seem then that at the moment when pupils reached the limit of their memory span, they over-estimated the number of digits ; but if the experiments had been continued with a greater number still, the pupils would have under-estimated the number.

Method of Correcting the Observations and Counting the Errors.—The method of correcting and counting the errors has an important bearing upon the number and the classes and so must be given in some detail. Any treatment of the errors will necessarily be more or less arbitrary ; but that treatment which gives the most uniform results and can be the most consistently applied would seem to be the least arbitrary. Some account too must be taken of the number of errors. The least possible number of changes necessary to make a given number correct was made, and each change was counted an error and classed an error according to its kind. The numbers were corrected as far as possible by restoring the order, before resorting to the substitution of other digits. The digits not required were then cut out and the proper substitutions made, the last process being to supply the vacant places with digits. It would be possible, however, to correct any number by either cutting out digits or substituting others. This would reduce the number of classes of errors but would greatly increase their number and destroy the distinction between the classes, which is based upon three different psychological processes. After careful consideration this method seemed to give the most uniform results and was the easiest to apply consistently.

The Frequency of the Different Classes of Errors.—To determine the class of errors which was first to occur, all those observations in which only one kind of error was made, were examined on 105 tests from the fourth and fifth grades.

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[1] Studies of Rhythm, by Professor G. Stanley Hall and Joseph Jastrow. Mind, Volume XI., No. 41.

TABLE VIII.—Showing the different classes of errors.

	Inversions of Order.	Substitutions.	Over Estimations.	Under Estimations.
Five-place Numbers . .	10	7	3	1
Six-place Numbers . .	44	15	15	5
Seven-place Numbers .	40	26	13	2
Eight-place Numbers .	33	10	14	4

If we take observations with a much greater number of digits, we should expect that the greatest number of errors would result from the dropping of digits. This matter will be discussed again further on. The total numbers of errors of every class in the Oxford Street School, when compared with the total number of observations, give the following percentages: Inversions of the order, 53%; substitutions, 31%; under and over estimations of the number of digits 15%.[1]

Position of the Error.—In what places are errors most likely to occur, or in what places are the digits most often forgotten? We encounter here a new difficulty in deciding which digits are incorrect. This difficulty applies only to digits that are interchanged. Then, too, when the pupils have left out figures they frequently indicated them by leaving a blank space. The first three digits are frequently present in their proper order, then follows a blank space and the last two in their order. The last three may be present and the rest absent or *vice versa*. To avoid this difficulty, all the proper digits that stood in their proper order either from the beginning or the end of the number, were considered correct and all others incorrect. 300 observations from the senior class in the High School on six, seven, eight and nine-place numbers and 300 from the eighth and ninth grades of Freeland and Woodland Street Schools have been examined with reference to the place in which the errors were most likely to occur. The errors occurring in each place were counted and compared with the possible numbers. The percentages were marked upon six ordinates for six-place numbers, on seven for seven-place numbers and so on, and these points connected by lines. The curves thus obtained will represent the relative frequency with which an error occurs in each place. Chart III. shows the curves for the Grammar School grades and for the High School. Those for the Grammar School are represented by the unbroken line and those for the High School by the dotted line. All the curves show a gradual though not uniform rise from the first place to one place past the middle; the curves then fall at first slowly, then rapidly, until they reach a point in the last place almost as low as at the beginning, in two cases lower. If observations with more than nine digits were made, we should expect that the pupils would be likely to forget the first more often than the last, so that it is not a mere accident that the curve for nine-place numbers ends lower than it begins. If fifteen or more digits were made the subject of observation, the pupils would probably forget all but the last two or three with only an occasional recollection of the fourth from the last and the first. There is a backward flow to the memory from the last, which affects quite perceptibly the second, slightly the third and in individual cases the fourth from the end. We have here a demonstration of the well-known rhetorical principle that the emphatic words in a sentence are the first and the last. In addition to this we get an idea of the relative importance of the other words. This will be true of any series of successive ideas. They are permanent in an inverse order as they are removed from the beginning except the last two or three which are permanent in their order from the last.

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[1] Important in this connection are the experiments of Dr. H. Münsterberg, *(Die Association successiver Vorstellungen*, Zeitsch. f. Psychol. Bd. I., H. 2, 1890.) He found that he could repeat seven letters without error, when they had been exposed one at a time in such a way that each was seen for one second; but he reached the limit of his power at ten. The most frequent error was that of substitution; inversions of the order were very rare. When, however, he solved problems in mental arithmetic aloud, while the letters were being exposed, his upper limit fell to seven, and only four or five could generally be recalled correctly. Instead of errors of substitution simply, errors of inversion of the order became the more frequent. The explanation he offers for this does not seem in the light of these tests to be conclusive. Possibly the difference in rate at which numbers and letters were given may help to account for the difference.

Conclusions.—I. The limit to the memory span for the pupils in the public school is six.

II. The memory-span increases with age rather than with the growth of intelligence. Experience in this matter is a better school than books.

III. The memory-span measures the power of concentrated and prolonged attention.

IV. Intellectual acuteness, while more often accompanied by a good memory-span and great power of concentrated and prolonged attention, is not necessarily accompanied by them.

V. The girls have better memories than the boys.

VI. With practice, pupils increase in their ability to remember groups of digits.

VII. Pupils unconsciously remember digits that they heard a day before, when they are used second time.

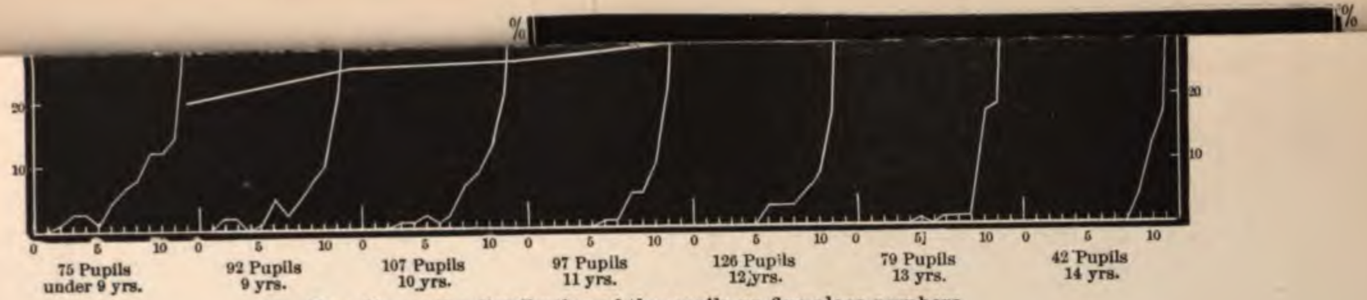
VIII. The tests do not show that the pupils suffer fatigue from the day's work. This fact shows that the work in the schools is probably not excessive.

IX. Memory images pass through three stages in leaving the mind. First, they suffer a confusion of order; second, a loss of certain elements and the substitution of associated elements; and third, a complete loss of some elements and no recovery.

X. Ideas previously in the mind and association forms of ideas are factors in causing the confusion of the memory image and its final loss.

XI. There is an apparent tendency to over-estimate the number of ideas presented to the mind, when the number of ideas is slightly greater than the memory span; but the general rule is to under-estimate the number.

XII. Ideas, except the last two or three in a series, are lasting in an inverse order as they are removed from the beginning of the series in which they occur. The last two or three are lasting but in decreasing degree as they are removed from the end of the series.



SET IV.—Shows the distribution of the pupils on five-place numbers.

CHART I.—Shows the classification of the pupils according to age.

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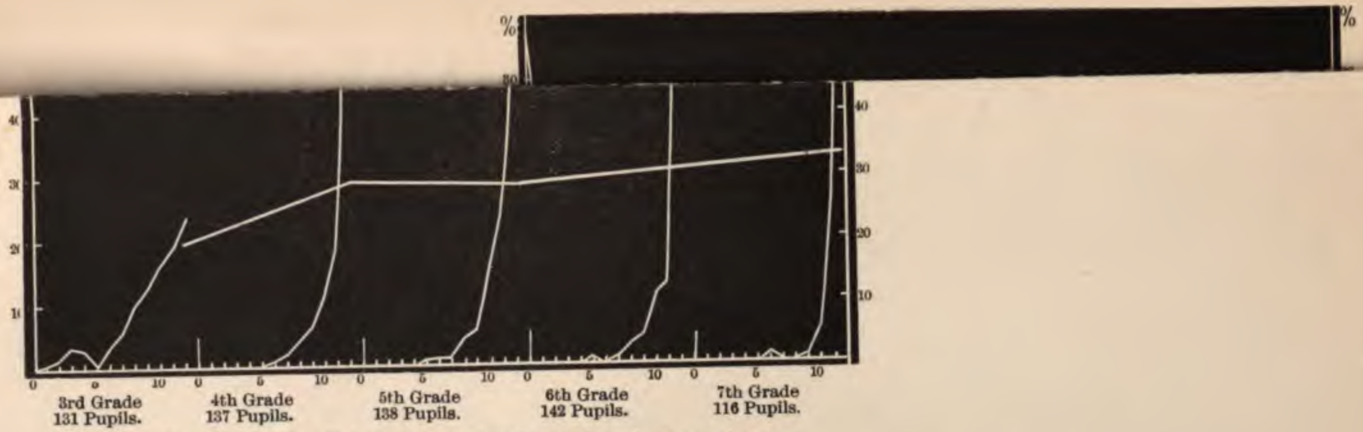

SET IV.—Shows the distribution of pupils on five-place numbers.

CHART II.—Shows the classification of the pupils according to grade.

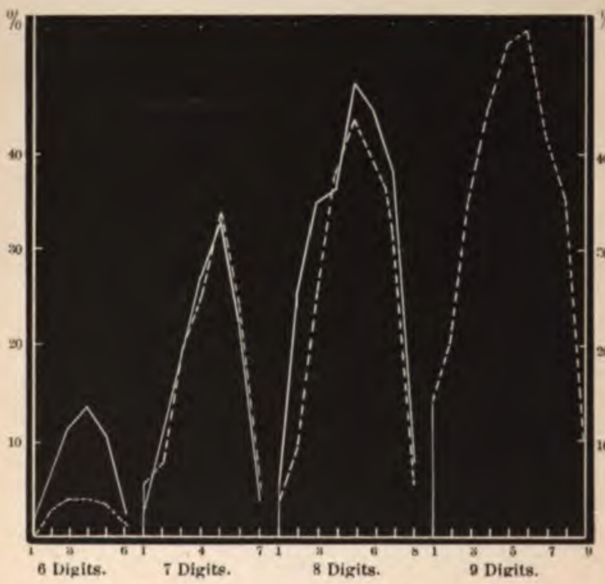

CHART III.—Shows the relative liability of a digit to be forgotten in any order in six, seven, eight and nine-place numbers. The unbroken line represents the distribution of errors for eighth and ninth grade pupils, and the broken line the distribution for High School pupils.

STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.—II.

BY JOSEPH JASTROW, PH. D.

A STUDY OF ZÖLLNER'S FIGURES AND OTHER RELATED ILLUSIONS.

(With the assistance of HELEN WEST.)

The present paper describes an investigation of an illusion which, while familiar and frequently studied, remains in its essence and conditions of origin quite unexplained. We make no claim of furnishing an adequate and final explanation, but simply aim to establish a few steps in that direction. The illusion is that so well marked in figure 1, first described by Zöllner[1]. In this figure the main lines appear very far from parallel; each adjoining pair of lines seems to converge at one end and diverge at the other. Here we have a complex form of the illusion involved, and it was our problem to ascertain the preceding members of the series of

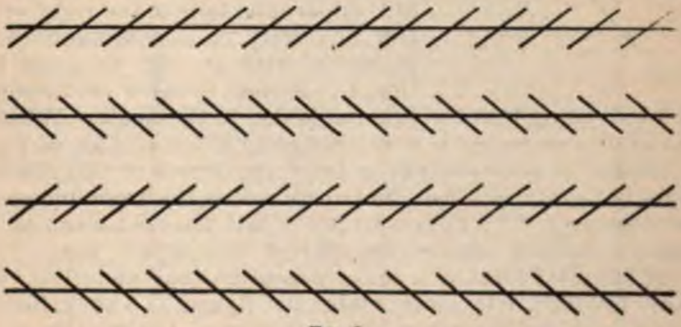

FIG. 1.

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[1] We are indebted for the use of figures 10, 11, 12, 13 and 24 to the courtesy of Messrs. Charles Scribner's Sons, publishers of Ladd, Outlines of Physiological Psychology, and for figures 1, 14, 15, 17, 18 and 25 to Messrs. Henry Holt & Co., publishers of James's Psychology, which courtesies we acknowledge with gratitude.

which this is the end term. It would be tedious to describe the various steps by which we stripped this figure of one and another of its complications, determining in a variety of ways what part they played in the total effect; it will be more acceptable to substitute for this rather laborious process an exposition beginning with the simplest type of the underlying illusion, and building it up step by step to its most complicated form.

When viewing two lines separated by a space, we are able to connect the two mentally and determine whether they are or are not continuations of one another; but if we add to one of the lines another meeting it so as to form an angle, the lines which seemed continuous no longer appear so, and those which were not continuous may appear so. In Fig. 2 the continuation of the line A appears to fall below the line B, and similarly the continuation of C apparently falls to the right of D. But in reality A is continuous with B, and C with D. If we cover the line C, A and B seem continuous; thus indicating that the illusion is due to the angle. What is true of obtuse angles is true, though to a less extent, of right angles and of acute angles; in brief, the degree of this illusion of discontinuity increases and decreases as the angle increases and decreases. The figures to prove this the reader can easily supply; further illustration thereof will appear later. This is the simplest form of a sense deception that underlies very many familiar but more elaborate figures. The principle therein involved we generalize as follows: Calling the direction of an angle, the direction of the line that bisects it and is pointed toward the apex, then *the direction of the sides of an angle will be deviated toward the direction of the angle.* A very important corollary of this main generalization emphasizes the point, that just as the deviation of direction is greater with obtuse than with acute angles, so also when

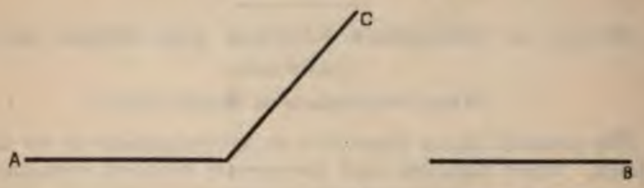
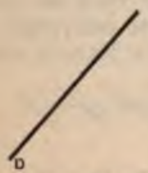


FIG. 2.



obtuse and acute angles are so placed as to lead to opposite kinds of deviation, the former will out-weigh the latter, and the illusion will appear according to the direction of the obtuse angle.

We proceed to notice a few of the means by which the illusion may be varied and tested. A relatively large distance between the lines, the continuity of which is to be judged, produces a more marked illusion than a relatively small distance. The appropriate figures the reader can readily supply. In other words, opportunity must be given for the eye to lose itself in passing from the one line to the other. The degree of illusion may be increased by increasing the number of angles in various ways. We may draw a series of oblique lines parallel to the line *C* (in Fig. 2) and joining the line *A*. Or again we may draw a line parallel to *C* from the left-hand end of line *B*. This gives Fig. 3, in which the two horizontal lines seem to be on entirely different planes. The direction of the deviations induced by these angles being opposite in tendency, the result is quite marked. Again we may add a second line parallel to the real continuation, which will be the apparent continuation, and we may further strengthen the tendency to regard the non-continuous line as the true continuation by shading them alike or otherwise differentiating them. Again, we may draw this second line slightly oblique instead of parallel with the first line with good success; this is done in Fig. 4. Both of these tests can be made

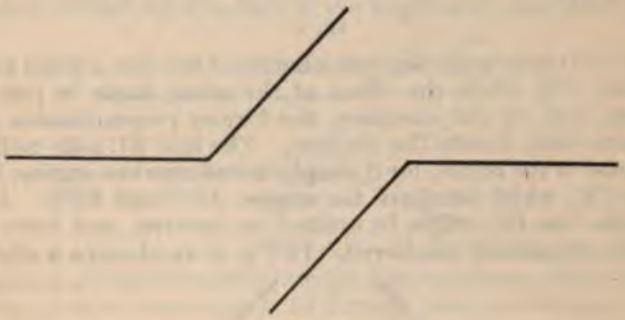


FIG. 3.

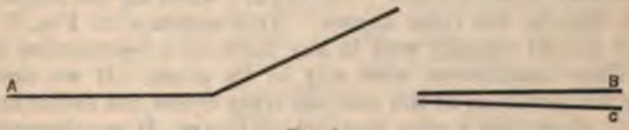


FIG. 4.

accurate by measuring the maximum deviation between the parallels or between the parallel and the adjacent oblique line, which the eye will tolerate and still retain the illusion of the false continuation; or again the angle alone might be drawn and the error measured, which the subject would make in adding what appears to him a true continuation of the sides.

On the basis of the general principle above enunciated, we may proceed to the explanation of a series of more complex figures. We turn to Fig. 5. Here the effect of the obtuse angle *ACD* is to make the continuation of the line *AB* fall below the line *FG*, while the effect of the acute angle is just the reverse, but, by our corollary, the former preponderates over the latter and directs the illusion. The line *EC* adds nothing essential to the figure, for it simply introduces two angles, *ECB* and *ACE*, which reinforce the angles *ACD* and *BCD*. Likewise the line *BC* might be omitted or covered, and leave the illusion essentially unaltered. In Fig. 6 we observe a slightly different form of the illusion, the continuation of each line appearing to run below that of the other, so that these continuations would meet at an obtuse angle. All these variations follow from the dictum that the direction of the side of an angle is deviated toward the direction of the angle.

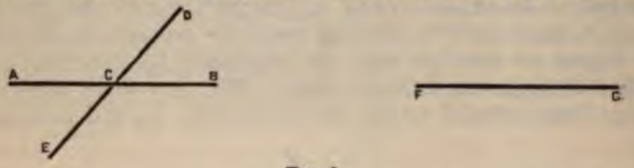


FIG. 5.

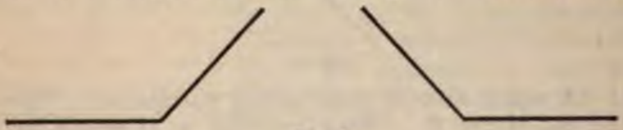


FIG. 6.

We may further note those cases, in which the effect of each angle is counteracted by that of another, resulting in the disappearance of all illusion; this occurs when all the angles are equal, that is, are right angles. This appears in Fig. 7 and would appear equally well in any form of a rectangular cross with lines continuous with any of its arms. If we omit or cover the portions of the vertical lines below the horizontal in Fig. 7, we obtain a very instructive figure. If we observe the horizontal lines, we notice that they do not appear perfectly horizontal, but each appears to tip upwards slightly from the apex; i. e., is deviated toward the direction of the angle; so also if we observe the vertical lines, we notice that they do not appear exactly vertical and parallel, but the right hand line tips slightly toward the right, the left hand line toward the left; i. e., they are likewise deviated toward the directions of their angles. This tendency of the sides of an angle to be deviated toward the direction of the angle, may result not only in making continuous lines appear discontinuous, but also in making parallel lines appear to diverge from parallelism.

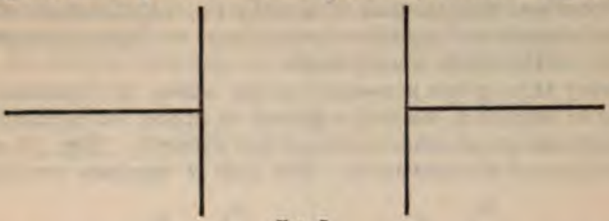


FIG. 7.

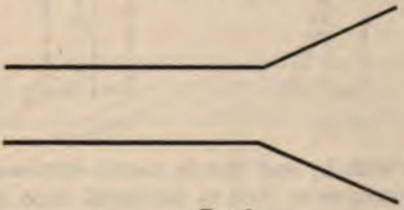


FIG. 8.

We may further illustrate the relation of divergence from continuity to divergence from parallelism by rotating the right half of Fig. 3 through 180°, and placing it under the left half. In this way we obtain Fig. 8, which shows that the same angles as readily produce slight deviation from parallelism as from continuity. To strengthen this illusion, we multiply the number of oblique lines and thus of obtuse angles. In so doing, we unavoidably introduce acute angles, but as before their effect is out-weighted by that of the obtuse angles. We thus obtain Fig. 9, in which the parallel lines diverge markedly above

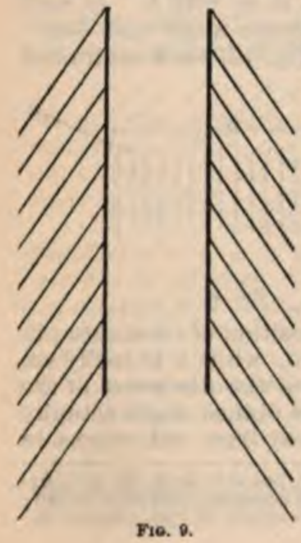


FIG. 9.

and converge below. If we now carry the diagonal lines *across* the vertical ones, the illusion remains, and it is clear from our dictum that it should (v. explanation of Fig. 5.) By simply adding more main lines, we have the figure of Zöllner, with which we set out[1].

Having thus given a resumé of the series of illusions from simple to complex, we may proceed to apply our principles to the explanation of other forms of the illusion. Fig. 10 shows the illusion of discontinuity; the line *a* appears continuous with *c*, but is so with *b;* and this is neatly emphasized in Fig. 11, in which a continuous line is deviated once in one direction and again in the opposite; the use of rectangles, instead of pairs of vertical lines, makes no essential difference. Fig. 12 presents the same illusion with the lines horizontal, the line *a* appearing continuous with *c*, while it is so with *b*. In each case the obtuse angle out-weighs the acute angle and determines the direction of the deviation. Fig. 12, when contrasted with Fig. 13, shows the effect of the position of its angles; in the former, *c* seems continuous with *a*, while *b* is really so, because the lower obtuse angle attracts the deviation of the line *c* towards itself; in the latter, the obtuse angle actually drawn between *c* and one of the vertical lines out-weighs in

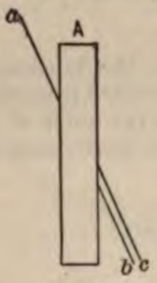


FIG. 10.

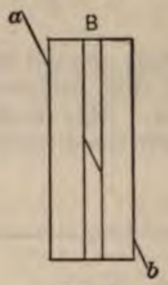


FIG. 11.

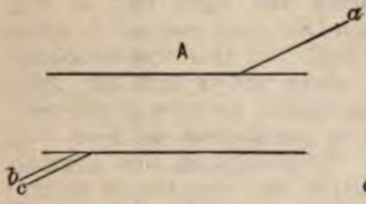


FIG. 12.

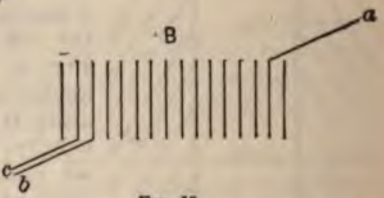


FIG. 13.

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[1] The oblique lines have been made shorter, but this does not add anything essentially different. It keeps the figure compact, and thus readily allows the judgment of parallelism.

effect the angle that is suggested between c and the horizontal line formed by the end-joints of the vertical lines, and thus the true continuation of a is below the apparent continuation. The divergence and convergence of the horizontal lines in Figs. 14 and 15 likewise follow from the above principles and illustrate some of its more complex forms, while most complex and brilliant of all is Fig. 16. In these figures the same line is made to deviate in opposite directions (by oppositely directed obtuse angles) from its centre, and thus theconverg-

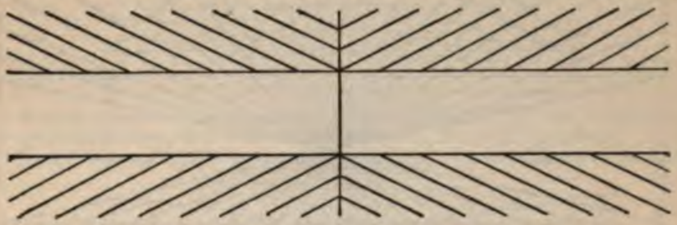

FIG. 14.

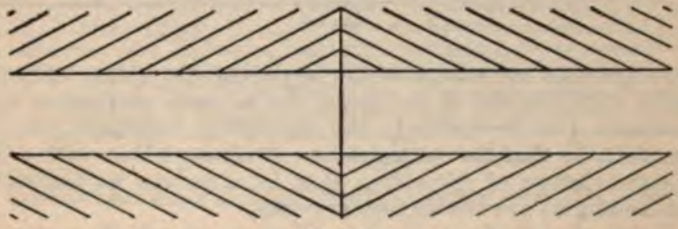

FIG. 15.

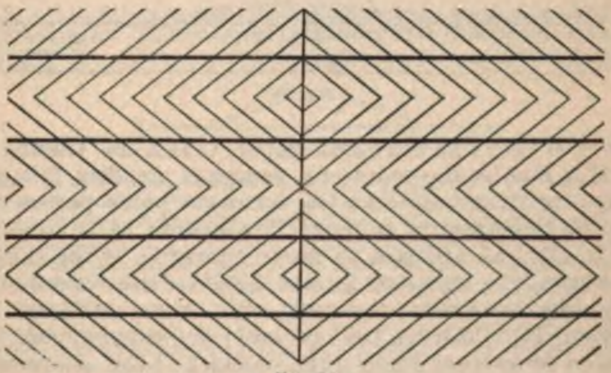

FIG. 16.

ence and divergence of the lines is greatly emphasized. The points at which the apparent change of direction occurs are also emphasized by cross lines. Fig. 17 adds the further principle that the extent of the apparent deviation varies directly with the size of the angle, for as each successive angle increases (or decreases), the deviation increases (or decreases), so that the straight line becomes a line with a continuous change of direction, that is, a curve; as before, the obtuse angles are the significant ones.

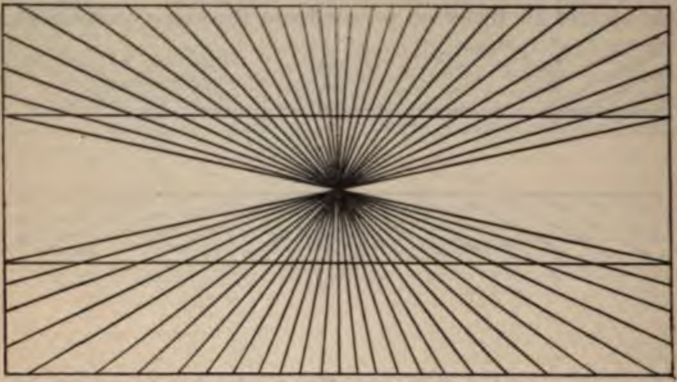

FIG. 17.

Helmholtz finds a similar illusion in which motion is involved and which Prof. James thus describes (Fig. 18.)

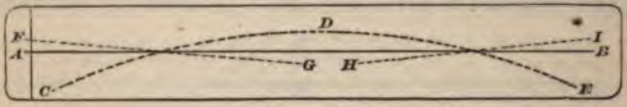

FIG. 18.

"Let A B be a line drawn on paper, C D E the tracing made over this line by the point of a compass steadily followed by the eye as it moves. As the compass point passes from C to D, the line appears to move downward; as it passes from D to E, the line appears to move upward; at the same time the whole line seems to incline itself in the direction F G during the first half of the compass's movement, and in the direction H I during the last half; the change from one inclination to another being quite distinct as the compass point passes over D." The line formed by the movement of the compass points acts as two oblique lines crossing the horizontal one. Curved lines produce the same illusion, as may be seen in Fig. 19, by the apparent sagging of the lines at the centre. The illusion is here strengthened by the presence of several curves.[1]

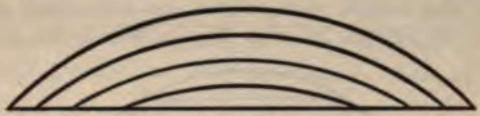

FIG. 19.

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[1] Wundt figures two illusions, which, apparently, are exceptions to our generalization, and which, accordingly, demand attention. In Fig. 20 the horizontal line appears as two lines tipping slightly downward

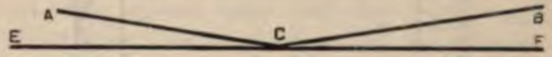

FIG. 20.

from the centre. Our first impulse would be to regard the illusion as due to the angles *ACE* and *BCF*, and we should, according to our dictum, expect the lines to tilt *upward* slightly. But remembering the greater effect of obtuse angles, we should view the figure as composed of Fig. 6, in which the two horizontal lines are approached to one another until they meet; when, by the effect of the angle *ACF*, *EC* is tipped down, and by the action of *BCE*, *CF* is tipped down. That such is really the natural way of looking at it will be evident from Fig. 14; at the centre of the upper line we have the very same arrangement of lines producing the same effect, and immediately in conjunction with the effective obtuse angles.

Wundt's next figure is more difficult to explain (Fig. 21). There the

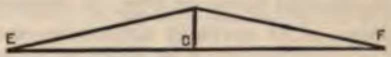

FIG. 21.

lines tip up at the ends, and there are apparently no angles which would make them do so. We have come to the view that this figure is a modification of Fig. 22. Here the obtuse angles are present and determine the

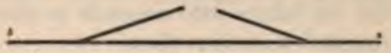

FIG. 22.

illusion. The oblique lines need not join to produce the effect, and the short vertical line, as in the other cases, simply brings out the point at which its change of direction takes place. We judge the tipping of the lines by reference to a horizontal which we carry with us or have suggested by the line of the page or the many horizontal objects we behold. We must likewise infer that the tipping in Fig. 21 is due to the obtuse angle formed by one side really present, and another suggested only by the continuation of *CE* and *CF*. The following considerations may serve to remove the artificiality of this explanation: (1) we do frequently judge by reference to imagined lines; e. g. our horizontal and vertical; (2) we use suggested lines in illusions; (3) the centre of Fig. 5 above presents Fig. 21 as the end term of a series, and in conjunction with the effective obtuse angle; (4) the illusion increases as this imagined obtuse angle increases, but decreases as the real acute angle increases.

The last illusion to which we shall attempt to apply our dictum is the familiar one whereby a square enclosed within a circle seems to bend in the circle at the four points of contact. That the circle contributes no essential part to the illusion can be shown in Fig. 23, in which the rectangle inscribed in the hexagon bends in the sides of the hexagon at the points of contact. Furthermore, the fact that the hexagon shows the illusion even better than the square, suggests that the larger angle is the more effective. This illusion we regard as a complicated form of Fig. 20 inverted. From our former explanation it is already clear why we should have a large obtuse angle at the point of contact instead of the actual continuous line. The portion of the curve adjoining the point of contact acts as a straight line, as it also does in Fig. 19. The effective angles are thus the angles of which *ABC* and *DBF* are types. The application of our generalization to forms of illusion not taken into account in the formulation is a very gratifying index of its value.

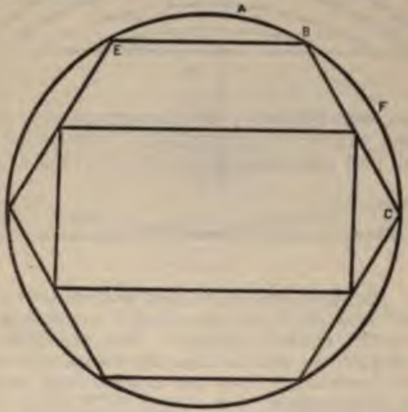

Fig. 23.

We turn finally to a brief account of the literature of the topic. Zöllner accidentally noticed the illusion on a pattern designed for a print for dress goods. He established the following points: (1) the illusion is greatest when the main parallel lines are inclined 45° to the horizontal; (2) the illusion disappears when viewed at a slight angle, as by holding one corner of the figure up to the eye; (3) it vanishes when held too far from the eyes to clearly see the cross striations; (4) the illusion is as good for one eye as for two; (5) the strength of the illusion varies with the inclination of the oblique to the vertical lines. In later studies he determined that (6) the angle between oblique and vertical lines at which the illusion is greatest is 30 degrees; (7) the illusion appears under the illumination of an electric spark quite as strongly as otherwise; (8) viewing it through red glass weakens it.

He also answers criticisms by Helmholtz and Hering. His explanation is curious and in its details unintelligible. He draws an analogy between these and illusions of motion and makes all depend on the view that it takes less time and is easier to infer divergence or convergence than parallelism.[1] Why the illusion should vary with the angle, under this theory, he does not explain; the fact that it is greatest at 45° he regards as the result of less visual experience in oblique directions. Apart from the fact that this theory does not explain and is not applicable to many of the figures, it can be experimentally disproved by a figure similar to Fig. 1 but with the lines actually inclined but apparently parallel, as suggested by Hering. Here really divergent lines all seem parallel, showing that the illusion does not consist of the inference of parallelism or non-parallelism, but of a certain angular distortion of the real relations of lines.

Hering *(Beiträge zur Physiologie*, 1861, pp. 69—80) added several of the figures above noticed (Figs. 14, 15, 19). He bases his explanation upon the curvature of the retina and the resulting difference in the retinal images of arcs and circles. He figures this explanation for the square enclosed in a circle and applies it to the rest. He criticises Zöllner and dismisses the fact that the illusion is strongest in oblique directions, as irrelevant. In a later article (Hermann, III., p. 373), he brings in the additional statement that acute angles appear relatively too large and obtuse ones too small.

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[1] It is well to note that Poggendorf called Zöllner's attention to a further illusion in his figure. This was printed in deep black lines, and the two parts of the oblique line crossing it seemed not quite continuous; *i. e.*, the illusion of Fig. 10, with a broad black line for the rectangle. Zöllner regarded this as unrelated to the other and accredited it to astigmatism.

He also prints a figure (Fig. 24) based upon the fact of greater illusion in oblique directions. This figure, as Aubert has pointed out clearly, refutes Hering's theory, for it shows a variation in the strength of the illusion, while the retinal image remains the same.[1]

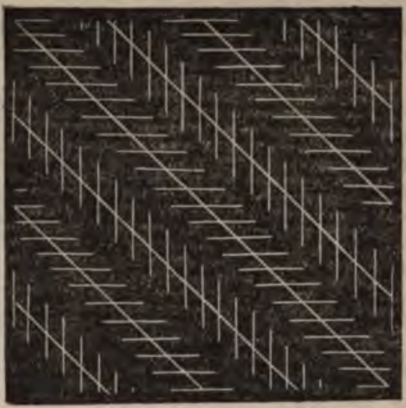

FIG. 24.

Aubert *(Physiologie der Netzhaut,* 1865, pp. 270—272) confines his attention to a notice of the results and views of others, closing with the sentence: "I am unable to give any explanation of Zöllner's illusion." In a later work, *(Physiologische Optik,* 1876, pp. 629—631), he practically repeats his former statements, and mentions that Volkmann explained it by an apparent alteration of the plane in which the oblique lines appeared; *i. e.*, they appeared in a plane inclined to that of the paper, and the inclination of the long parallel lines to this plane appears as inclination toward one another.

Classen *(Physiologie des Gesichtssinns)* after disagreeing with all previous theories, gives his own explanation in these words: "Now the cause of the illusion is clear: in recognizing the directions of the converging and diverging oblique lines, we judge them by their relations to the vertical ones. These recede from the oblique lines where they diverge, and approach them where they converge; and thus the direction of the verticals is regarded as a separation toward the side of convergence and an approaching toward the side of divergence." Classen noticed that the illusion appeared as soon as a pair of parallels was crossed by a pair of oblique lines which formed an acute angle at their junction. He insists that a pair of lines of opposite direction is necessary to produce the illusion, and leads one to infer that if this were not so his theory would be disproved. This can be shown in various ways; *e. g.*, by drawing only the left half of Fig. 9 and substituting a parallel line for the right half, the illusion remains, though not so distinctly.

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[1] Kundt (cited by Aubert) attempted to get an experimental proof of Hering's view, but his results at close distances, which alone are relevant, failed to corroborate the theory. Kundt also determined the relation between the size of the figure and the distance from the eye at which the illusion disappeared.

Lipps (*Grundthatsachen des Seelenlebens*, 1883, pp. 526—530) regards the illusion as primarily psychical; whatever parts the movements of the eyes play being determined by the attention. He says: "If we draw (Fig. 25) the line *pm*

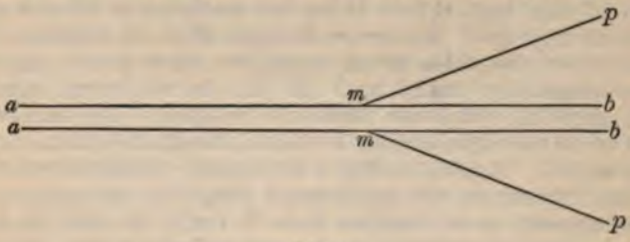

FIG. 25.

upon the line *ab*, and follow the latter with our eye, we shall, on reaching the point *m*, tend for a moment to slip off at and to follow *mp*, without distinctly realizing that we are not still on the main line. This makes us feel as if the remainder *mb* of the main line were but a little away from its original direction. The illusion is apparent in the shape of a seeming approach of the ends *bb* of the two main lines." Prof. James, whose words we have been quoting, adds: "This, to my mind, would be a more satisfactory explanation of this class of illusions than any of those given by previous authors, were it not again for what happens in the skin." Prof. James thinks that this class of illusions belongs to the field of sensation rather than of unconscious inference.

Hoppe (*Physiologische Optik*, 1881, pp. 73—83) gives a careful and critical digest of the views of others, which he finds it difficult to understand. His own is no less so but seems to be that our eyes and our attention are drawn to those lines that do most to fill out space, and that we run out the oblique lines until they meet; from this imagined point of junction the real parallels look divergent. When we carefully fixate the parallel lines, the illusion is avoided. Or again, "we follow the *filled* middle space according to the course of the oblique lines and *neglect* the black parallel straight lines; or, because the latter are thus less noticed and viewed from a greater distance, we strengthen the appearance of their separation by the indirect view we obtain of them." The illusion is not retinal, because it vanishes in the after image; it is intellectual in origin. It is difficult to see how Lipps's view would be expressed so as to apply to Fig. 2, or why the illusion should disappear in Fig. 7. Hoppe's view is open to the same objection as Classen's and is refuted by the same figures.

Wundt *(Physiologische Psychologie*, 3d ed., II., pp. 124—132) although bringing in other factors as well, makes his main argument rest upon the view that we tend to overestimate acute and underestimate obtuse angles. He gives no proof of this fact, if fact it be, nor explains in what manner the error appears. He seems to mean that, in judging of the direction of the sides of an angle, we view acute angles as larger than they really are. If this be so, there must be some angle at which the illusion disappears, and this would seem to be the right angle; however, we get the illusion with right angles. Again, in Fig. 5 and many similar figures that we could construct, the acute angle judged by the same means would appear to be smaller than it really is, and in many respects acute and obtuse angles are affected alike. In common with others, Wundt regards the increase of the error at 45° as due to a less exact visual experience.

Pisco *(Licht und Farbe*, 2d ed., 1876, p. 268) gives no explanation, but adds the beautiful Fig. 16.

Helmholtz *(Physiolog. Optik*, pp. 564–574) presents a peculiar view of the subject. He begins with the illusion of the deviation in direction of the two parts of an oblique line separated by a rectangle and regards the particular cause of this illusion to be the curving in of the oblique lines as they meet the sides of the rectangle or heavy vertical lines. Moreover, this is especially true of small figures, in which as a whole the illusion is more marked. This deviation, then, at least in small figures, is due to irradiation. He supplements this explanation with one that will apply to large figures and to Zöllner's illusion. He says: "We may consider these illusions as new examples of the law above indicated, according to which acute angles, being small in size and clearly limited, appear in general as too large when compared with right or obtuse angles." Moreover, movement plays a large part in at least some of the figures, and in these the illusion disappears under precise fixation and the electric spark. This effect of movement is illustrated by the instance cited above and leads to a sort of contrast whereby a clearer difference seems a larger one. Besides the general objection that so many principles are brought in to explain facts so clearly belonging to one sphere, and the further objections which have already been advanced against the alleged overestimation of acute angles, several detailed criticisms might be made. In the first place, Helmholtz has not shown that small figures present the illusion better than large ones; in his figures he has drawn less than half as large an acute angle in the small figure as in the large one, and this is the cause of the difference he observes. Regarding the alleged curvature of the lines, it is difficult to see it; and it, as well as the possibility of irradiation, may be eliminated by drawing all the lines light and not allowing the oblique line to quite meet the vertical ones—under such circumstances the illusion persists. Helmholtz's chief argument for the effect of fixation is drawn from the heavily-drawn form of Zöllner's figure, in which he looks at the white bands with oblique lines running out like the feathers on an arrow, and *sees them parallel:* but this is precisely what *must occur* from the position of the angles, the effect of each angle being compensated by another. The two modes of drawing the figure make two figures of it. The arguments from the electric spark experiments are certainly questionable both in fact and inference, and it must be admitted that the entire treatment is unsatisfactory.

It will be seen that the field we have entered is a very complex one, and that a most important problem—Why do we deviate the sides of an angle toward the direction of an angle?—remains to be solved. How far does this depend on eye movements, how far upon inference, and the like? The chief defect of former attempted explanations seems to us to consist in theorizing upon too limited a range of facts. What is true of one group of figures fails to apply to others. Before an explanation can be satisfactory we must know precisely what it is that we are to explain, and this necessitates a correct and comprehensive generalization of the facts: this it is that we have attempted to supply.

Our study of these illusions leads us to regard them as essentially psychological in origin; they are illusions of judgment and not of sensation. Furthermore, we would regard them as an outcome of the general principle that we are prone to judge relatively rather than absolutely; that our perceptions differ according to their environment; that a sense impression is not the same when presented alone and when in connection with other related sense-impressions. A line presented by itself is a different object from a line as a part of an angle or of a figure. However much we desire to consider the line independently of the angle, we are unable to do so. We have the direction of an angle and the direction of the lines that form the angle, and we are unable to consider the latter absolutely without reference to the former. The more nearly the directions of the angle and of the sides coincide, i. e. the smaller the angle, the smaller will be the error induced by this relative mode of viewing the lines. The whole series of illusions would thus be subsumed under the law of contrast or better of relativity; and the different variations and degrees of the illusion would find their explanations in the readiness with which they suggest and enforce misleading comparisons.

In order to exhibit a type of illusions most readily explicable from this point of view, as well as to exemplify the suggestiveness of the latter, we will consider an allied group of usual illusions.

Just as the presence of angles modifies our judgment of the *directions* of their sides, so too, the angles will modify the apparent *lengths* of lines. This form of contrast is most strikingly exhibited in Fig. 26, and best by comparing I and IV, i. e. cover up II and III. It seems almost incredible that the horizontal portions of I and IV are of equal length, and yet such is the case. II and III supply the intermediate steps, and in comparing the four figures the horizontal portions seem to become successively shorter from I to VI, while, in reality, they are all one length. Here, again, the greater the angles formed at the extremities, the greater the apparent

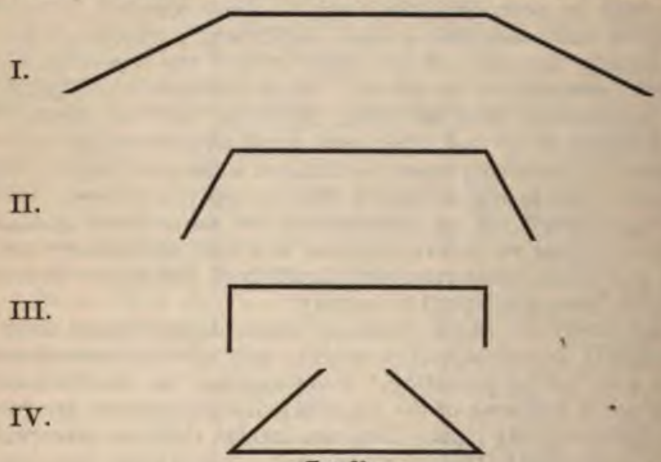

FIG. 26.

length of the line; and thus the constrast is greatest between the very obtuse and the acute angles. Other factors contribute to the illusions; e. g. the positions of the figures, the juxtaposition of certain lines, the distance between the figures, and the like. The illusion persists if the horizontal lines be omitted, and we judge the spaces between the oblique lines. It also shows very well by cutting the figures out of paper either as they are or as truncated pyramids (by joining the ends of the oblique lines by a line parallel to the horizontal one), and viewing them against an appropriate back-ground.

We may also be tempted to judge of two areas by their juxtaposed lines, thus regarding one of two equal areas as larger than the other. This is shown in Fig. 27, which also

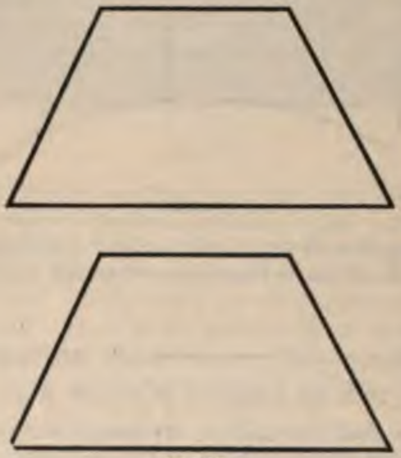

FIG. 27.

shows very well when the figures are cut out and moved about to assume various positions. The upper figure seems larger, because its long side is brought into contrast with the shorter side of the other figure. Similarly, a square resting on a corner seems larger than one resting on a side, because we then contrast the diagonal with the side. Fig. 28 on the following page presents another illustration of the same principles; the lower figure seems to be distinctly the larger, and the contrast is emphasized because it is thrown entirely to one side of the figure. In judging areas, we cannot avoid taking into account the lengths of the lines by which the areas are limited, and a contrast in the lengths of these is carried over to the comparision of the areas. We judge relatively even when we most desire to judge absolutely. Relative distinctions and the perception of relation seem to be more natural and significant than absolute ones. We cannot view the part as unrelated to the whole. This is a widely applicable principle and is suggested as a convenient guiding principle by which the study of such illusions of sense may be profitably directed.

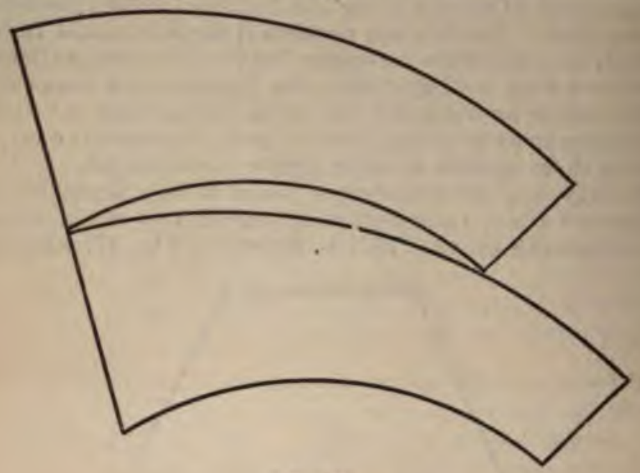

FIG. 28.

A STUDY OF INVOLUNTARY MOVEMENTS.

(With the assistance of HELEN WEST.)

The dictum that thought is repressed action most readily finds illustration in conditions of the nervous system varying somewhat from the normal. It is easier to detect the action of not definitely recognized laws in extreme forms than in average ones. The modern view of morbid action, however, emphasizes the close relation of the abnormal to the normal; there exists in the latter in germ and to a limited extent, what is full grown and characteristic in the former. If, under great excitement and extreme fascination of the attention and in favorably constituted individuals, the involuntary movements are pronounced, the rudiments of these movements should be demonstrable in the average individual under normal conditions. For this purpose delicate apparatus may be requisite, and a variable amount of success is to be expected. The question of apparatus is of importance, and our present study aims to do little more than describe the apparatus and illustrate what results may be obtained therewith.

The apparatus is so simple that a brief description will doubtless be sufficient to convey a clear idea of its mode of action. There is first a piece of plate glass (see Fig. 29) fifteen inches square, resting in a stout wooden frame; this frame is mounted on three adjustable brass legs, raising it an inch or so from the table. By means of the screw-adjustments of the legs, the plate glass is brought into exact level. Three brass balls, which must be very perfectly turned and polished spheres, about three-fourths of an inch in diameter, are placed in the form of a triangle upon the plate; upon these balls rests a very light crystal-plate glass, fourteen inches square, mounted in a light wooden frame. On the upper surface of this plate is placed a piece of paper to hide the balls, and on the paper we lightly rest the finger-tips of our hand. It is almost impossible to keep the plate from all motion for more than a few seconds; the slightest movement of the hand slides the upper plate upon the balls. To maintain the apparatus in working order it is necessary to keep the glass and balls well polished by rubbing with a cloth and a little oil.

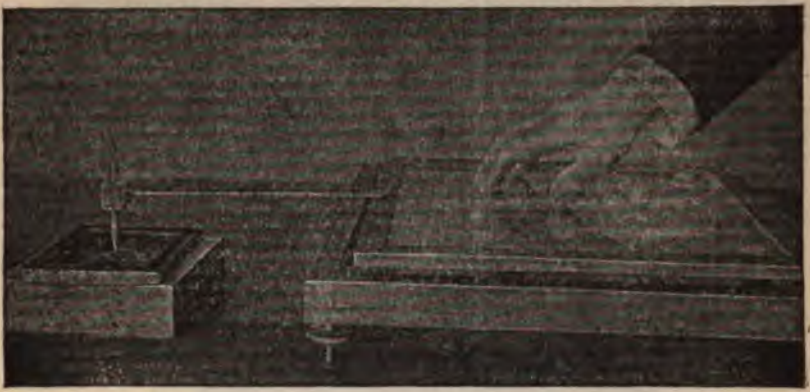

FIG. 29.

The recording of the movement is equally simple. To the light frame is attached a slender rod about ten inches long, bearing at its end a cork; piercing this cork is a small glass tube and in the tube there is a glass rod snugly fitting the tube and drawn to a fine point. The point of the rod traces the movement of the hand with great accuracy, and, not being rigidly fixed, can accommodate itself to all irregularities of movement or of the writing surface. A piece of smoked paper stretched over a glass plate, upon which a record is made, and a large screen to prevent the subject from seeing the record, complete the apparatus. This apparatus enables us to record all movement in the horizontal plane, and, inasmuch as its chief purpose is to write slight involuntary movements, we have given it the name of the *automatograph* and may speak of such a record as an *automatogram*.

The type of an experiment is the following. The subject places the finger-tips of his extended right hand upon the glass; he is told to hold the arm still and pay no attention to it. He is asked to read some lines or colors, or to count the beats of a metronome; this naturally engages his attention. When all is in readiness the operator drops the glass rod into the tube, and the record begins. When the subject has been occupied in this way for a minute or so, we have, as a rule, a very clear record of the direction of his attention in the automatogram.

In order to have a test by which to compare the relative sensibility of different persons for movements of this kind, we arranged to have a number of persons go through a series of tests, a typical result of each of which will be figured. Each experiment occupied from about three-fourths to two minutes, and when possible we noted the progress of the record for each 15 or 30 seconds.

A series of patches of color 5x20 mm. were placed in horizontal rows on a vertical wall about ten feet distant. The subject was required to read aloud the names of the colors. The general tendency is for the hand involuntarily to move toward the colors with a variable degree of constancy, rapidity and directness. An average result is shown in Fig. 30. We have another record, lasting but 45 seconds, but covering 6½ inches, which in extent and directness is the most remarkable of our records. The appearance of the line is similar to that of Fig. 30, but with several points at which the line is almost directly toward the colors.

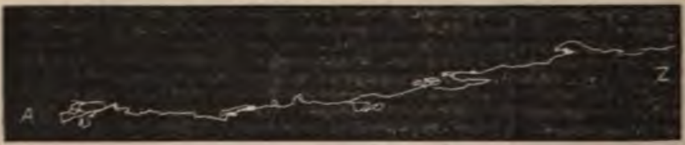

FIG. 30. →

Reading colors. Time, 95 seconds. A indicates the beginning, and Z the end, of each record. The arrow everywhere indicates the direction in which the object attended to was situated. When the numbers 1, 2, 3, 4, occur, they indicate the point of the record at 15, 30, 45 and 60 seconds after the beginning of the record.

On two occasions the subject who gave us this striking record evidenced the action of the attention in another, equally striking way. There were three rows of colors which were read; the first one from left to right, the second from right to left, and the third from left to right; the involuntary movements correspond to the movement of the attention, as is vividly shown in Fig. 31.

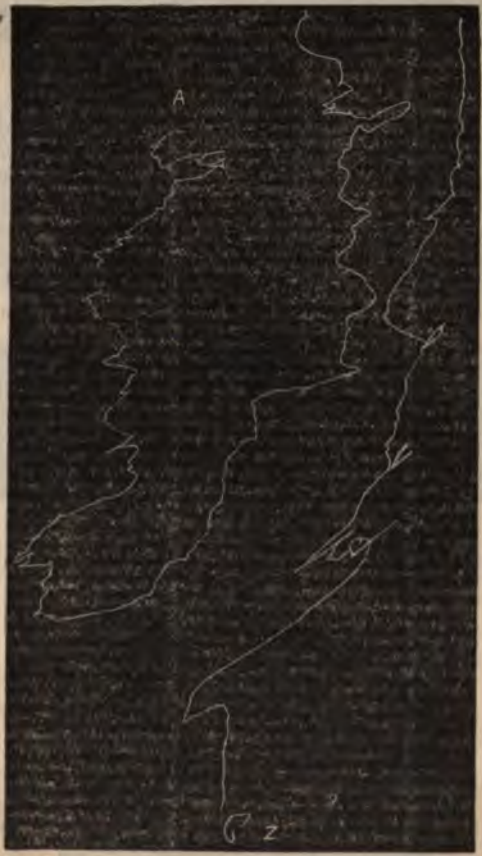

FIG. 31. →

Reading three rows of colors; the movements closely following the attention. Time, 90 seconds.

These are certainly striking proofs of the ease with which, in sensitive subjects, the hand involuntarily follows the movements of the eye.

A second test consists in substituting the reading of a printed page for the colors the results are quite similar. Fig. 32 represents a typical result.

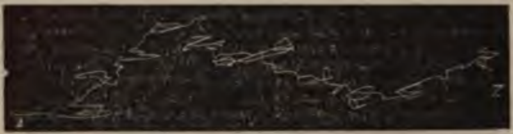

Fig. 32. →
Reading from a printed page. Time, 45 seconds.

We next pass on to cases in which the attention is directed to sounds. We set going a metronome, and ensured the subject's attention to it by having him count the beats. The usual rate was 140 strokes per minute. Here, again, we find two types of involuntary movement: the one a moving toward the sound, represented in Fig. 33; the other a keeping time with the beats, not accurately at all, but in a general way, as is shown in Fig. 34. When we consider how strong is the tendency to keep time to enlivening music, it will not surprise us that we are able to record these slighter and more unconscious movements to simple time beats. We frequently performed this experiment by placing

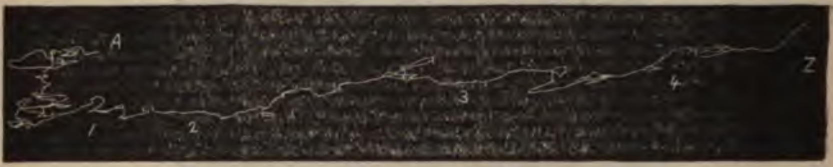

Fig. 33. →
Counting strokes of metronome at 140 per minute. Time, 70 seconds. Also illustrates slight hesitation before movement towards metronome begins.

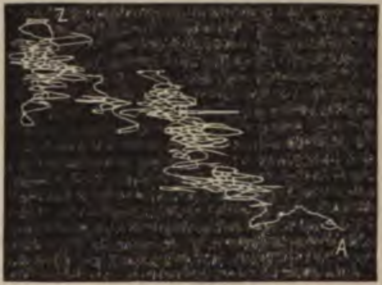

Fig. 34. ←
Counting strokes of metronome. Shows movements to and fro with the strokes.

the metronome first in front of, and then behind the subject, and the contrast between the direction of the lines is, as a rule, quite striking.

We recorded a similar experiment for sight, by substituting for the metronome a silently swinging pendulum, the oscillations of which were to be counted. Again we observe the two kinds of records, the second, as before, being considerably less frequent than the first. These are given in Fig. 35 and Fig. 36. A pair of records derived from this form of experiment well illustrates the extremes of rate of movement: one subject moved 11 inches in two minutes, another 1⅜ inch in the same time, though in both cases the motion was regularly toward the point of attention, the swinging pendulum.

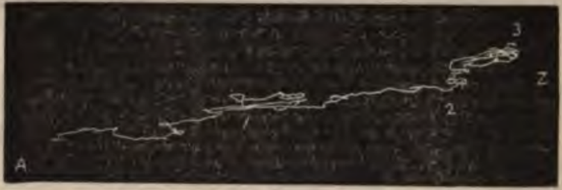

FIG. 35. →
Counting oscillations of a pendulum. Time, 45 seconds.

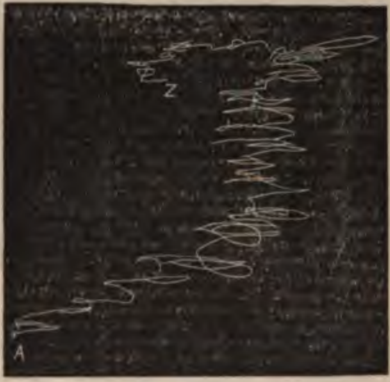

FIG. 36. →
Counting pendulum oscillations; shows movements to and fro with the oscillations. Time, 80 seconds.

Our next experiment approximates closely that of the muscle reader. We directed the subject to hide a knife at some part of the room not near the center, and immediately thereupon took a record upon the automatograph, the subject thinking of the knife. In some cases this experiment was unusually successful, in others fairly so; the direction of movement usually closely approximated the direction in which the knife lay. Fig. 37 represents a fair result. A quite similar experiment consists in directing the attention to some prominent building or locality in the neighborhood, not by actually looking toward the place, but by voluntarily thinking of it. We have many very excellent examples of such records. Fig. 38 will serve as a type of the more successful ones.

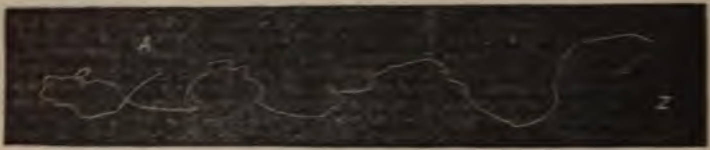

FIG. 37. →
Thinking of hidden object. Time, 30 seconds.

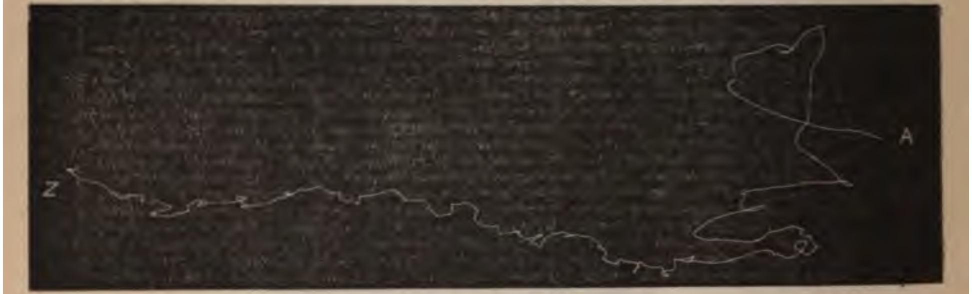

FIG. 38. ←
Thinking of a building in the direction of A to Z. Time, 120 seconds.

This does not exhaust the methods of attracting the attention but it illustrates our chief modes. Reading to a person from different parts of the room is often successful. Quite an interesting form consists in having the subject's attention change in the course of an experiment to different localities, as by having him read from a book carried about by an assistant. Such a result is shown in Fig. 39, in which we have an irregular figure closing in upon itself and clearly indicating the circular movement of the book.

We often succeeded in distracting a subject's attention by a noise in another portion of the room, the hand moving toward the source of the noise. We also recorded the involuntary start that occurred when a ball was suddenly dropped upon the floor.

The figures given will sufficiently illustrate the nature of the results obtainable with the aid of our automatograph and it remains only to notice a few general points regarding them.

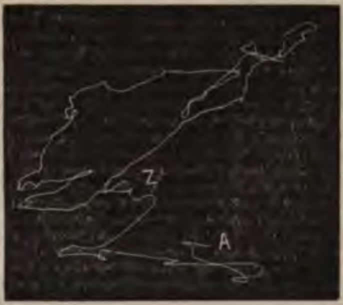

Fig. 39. ↻ Reading from a page moved about in a circle.

It would be interesing to determine by this method the relative degrees of muscular accompaniment for these different kinds of attention; but our methods are not as yet sufficiently refined to solve this problem; the result seems to vary with individuals and with the sense organ engaged. As a preliminary result it may be worth recording that a number of measurements yielded an average rate of movement of about two inches to the minute toward the object thought of.

Of great importance is the nature of the individual differences in these experiments. Our normal experience would naturally anticipate a difference about as characteristic at least as that of hand-writing. Any minute discussion of the point would be obviously premature, but in general it seems possible to arrange these differences in types. We should distinguish between those who move rapidly and directly, and those who move slowly and circuitously; between those in whom the movement quite exactly follows the line of attention, and those in whom it does so only approximately or irregularly. Instances of such distinctions have already been indicated.

We add Fig. 40, which may be contrasted, in regard to the

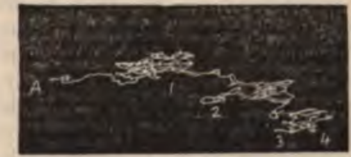

FIG. 40. →

Counting oscillations of a pendulum. 1, 2, 3, 4 indicate the points 30, 60, 90 and 120 seconds after the start. Time, 120 seconds. Illustrates small and indirect type of movement.

character of the curve with Fig. 31; the latter shows directness of movement and great extent, the tracing rarely becoming confused by the hesitancy of the subject, while, in Fig. 40, the movement is slow and the record involved by continual retracings of the path of movement. Another important distinction relates to the time at which the most significant movements occur, and mainly whether the first impulse is toward the object of attention followed by much hesitation of movement; or whether at first there is little movement followed, after fatigue, by the movement determined by the attention. We have many more of the former type than of the latter; one of the former is presented in Fig. 41. Figs. 33 and 38 partly illustrate the reverse tendency. We might further distinguish between those subjects who show the direction at each portion of the tracing and those who show it only here and there.

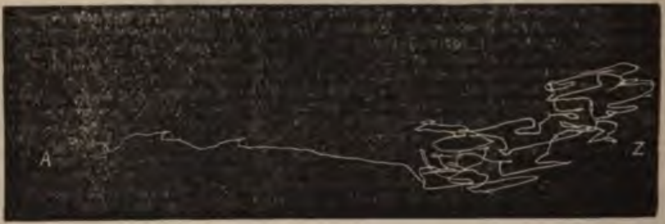

FIG. 41. ⟶

Counting beats of metronome. Illustrates first impulse toward object of attention followed by hesitation. Time, 90 seconds.

These types may possibly suggest what kinds of involuntary movements best subserve the purposes of the muscle reader; all alike illustrate the general line of tendencies which he utilizes.

A very natural query relates to the possible influence of the position of the arm and body, and also of such other factors as the pulse and respiration upon the character of the tracing. The main distinction in regard to the position is whether the arm is (1) held straight out from the shoulder in a line with the trunk, or (2) at 90° from this position, or (3) in an intermediate position. In the first of these, movements toward the front are obviously easier to make than movements toward the rear, and in both (1) and (2) movements toward the body are easier than those away from the body. By changing the position of the object to which the attention was given, we could thus favor or interfere with the tendency to involuntary movement in certain directions. In our experiments, we allowed the subject to assume a natural and comfortable position, which was usually an intermediate one, with the arm not fully extended. This position allows movement in all directions, though it is still true that movements toward the front and toward the body are favored above movements toward the rear and away from the body. The direction of the attention is thus sometimes partially obliterated in some subjects, but in most of them it appears in spite of this tendency. This factor deserves a more special investigation. While respiration may have some effect, we are inclined to regard it as very small, and the pulse as not entering into consideration at all; for, in order to get the tracing of pulse and respiration (by other apparatus) with equal distinctness, we had to magnify them very considerably.

The question of the precise significance of these movements is largely dependent in the testimony of the subjects. While there are individual differences in this as in all other respects, the consensus of the verdicts might be thus expressed: at times we become aware that our hand has moved, but rarely of the direction of its movement; the movements are sometimes unconscious but always involuntary, there is often great surprise at the result. The one objective test we could apply was to intentionally simulate these movements and the result was measurably different from the genuine involuntary record.

It is hardly necessary to enlarge upon the bearings of these experiments upon the processes of muscle reading and kindred phenomena. They indicate the close connection of mind and muscle, and in demonstrating the extent of recordable automatic movements, suggest the many other and subtler means by which we may give to others some notion of what is going on in our own minds.

Observations on the Absence of the Sense of Smell.

(With the assistance of Theodore Kronshage.)

The subject of our observations is a Mr. E., aged 21 years, a student of this University, who is deprived of all the sense of smell. The defect is probably congenital and of nervous origin. As Mr. E's. knowledge of his defect was denied from such occasions as would occur in every-day life, our first step was to test the degree and extent of the anosmia. We approached various substances to his nose asking him to inhale them and report the result; we tried in this way strong liquid solutions of wintergreen, bitter almonds, ether, alcohol, ammonia, cinnamon, camphor, etc. Camphor produced very slight if any sensation and the same is true of wintergreen and cinnamon. Bitter almonds, ether, and most markedly ammonia, produced a sharp, more or less stinging sensation in the nose. Alcohol was described as sweetish like perfumes. We next tried several substances in pairs to determine how far, when first told which was which (not by name but by calling one *A* and the other *B*), he could distinguish between them, and, as a check against unconscious bias, experiments in which one of the pair was distilled water were introduced. This precaution was quite necessary, for it happened that when bitter almonds and water were compared in this way he mistook them three times in five trials, though professing te get some sensation from the bitter almonds when presented alone. The water, however, was frequently recognized by its entire absence of any sensation of smell. Such distinctions therefore as are perceived by him are by no means altogether clear. With the pair, ether and water, eleven trials resulted in eleven correct answers, the point of distinction being that "ether opens up the throat like peppermint." With wintergreen and bitter almond, the latter yielding the distinctive effect, there were only 3 errors in 18 trials. With ether and ammonia both giving decided sensations but of somewhat different nature there were 2 errors in 8 cases; this may however have been due to over stimulation, as the substances used were so strong that neither of us could take them to the nose with comfort. Ammonia was described as immediately affecting the nose, ether as going back to the throat and affecting it. With wintergreen and cinnamon, neither yielding any definite sensation the result proved to be mere guesswork, and the same is true of cinnamon and camphor.

Inasmuch as the sensation arising from the inhalation of alcohol was described as similar to that of perfumes (alcohol being an ingredient of these) we ascertained how far the presence or absence of alcohol could serve as a means of confusion or indentification of substances. We made a strong solution of wintergreen and of cinnamon in alcohol, and from each of these Mr. E. obtained a similar but pronounced effect. The attempt to distinguish between the two, however, resulted in as many failures as successes. We next compared pure alcohol with wintergreen dissolved in alcohol, and no difference except of intensity was observed. To complete the proof we made a solution of wintergreen in water and in alcohol. The latter gave a distinct sensation, the former almost nothing. In all the eight trials the two were correctly distinguished.

The next point tested was whether distinctions of intensity within a perceived range of sensations were obtained. We tried strong and weak alcohol with the result that in all cases (eight) they were correctly distinguished from one another; the sensation was described as a "sweetish taste in the mouth."

In the above results indications were given that Mr. E. was less than normally sensitive to irritants. To measure this difference we determined how many drops of very strong ammonia must be added to 100 cc. of water (1) to produce a sensation, (2) to make it objectionably strong. We obtained the characteristic effect with but one drop in 200 cc. and even one in 300 cc.; while Mr. E. needed 2 drops in 100 cc. Eight drops in 100 cc. made it very objectionable to us, but he said it was like some perfumes, and it took 23 to 25 drops to produce an objectionably strong sensation.

We next tested the sense of taste. A preliminary survey served to show that the sense was present and presumably in a normal degree. To complete the test we compared his taste with ours for sugar, acetic acid and quinine. We found about the same measure of sensitiveness for Mr. E. and for us; and found nothing differing from the normal in any respect.

We proceeded to investigate those mixtures of smells and tastes, which make up most of the sensations obtained during eating. We took the ordinary flavoring syrups of commerce, lemon, vanilla, currant, orange, strawberry and raspberry. From all these Mr. E. obtained only a general sweetish sensation with no distinction between them except from the lemon which was in the main distinguished by the mixture of sweet and sour. He could in part tell them by their different degrees of sweetness, but when presented in proportions in which they seemed to us equally sweet all distinctions were impossible to him. The tests showed as many wrong as right answers. It so happened that some of these substances fermented and these he could at once detect as different from the others and also the more fermented ones from the less so. A series of candies with some of the above flavors yielded corroborating results. It should be understood that all these substances were tasted.

Next, a series of spices was tried with the following results:

Mustard; a sharp sensation on the end of the tongue; not recognized.

Pepper; same effect but stronger.

Coffee; not recognized, a slight taste.

Cinnamon; recognized, sweet and sharp.

Broma; sweet.

Cloves; recognized, taste distinct but not describable.

Thyme; sharp, bitter, something like cloves.

Tea; no effect at all.

Anise; sharp, bitter, unpleasant.
Caraway Seed: mild, sweetish, and salt.
Ginger; not recognized, burns.
Mustard Seed; burns decidedly.
Citron; recognized by its feeling on the tongue, sweet.

In brief some were recognized by secondary qualities, but those that we recognize by flavor were not differentiated. A separate series was tried with tea and coffee, and one with ginger and cloves. Neither of either pair was distinguished from the other; the latter were both called sharp but with no distinction between them.

We also tested Mr. E.'s temperature sense, at about 15°, 30° and 60° R. At all these points his sensibility was as good as ours, differences of 1° being everywhere recognized. The test was made by taking a mouthful of water heated to the required temperature and then throwing it out.

The great importance of these observations lies in the analysis they enable us to make of the complex of sensations obtained in the mouth and nose. In Mr. E.'s case taste is normal, the temperature sense is normal, the tactile sensibility is present (though as far as irritants are concerned, to a diminished extent,) while smell alone is absent. Accordingly we may conclude that such distinctions as Mr. E. fails to make are in us due to the sense of smell, and such as he makes are due to other senses. *The results conclusively show that a great many of the mouth-sensations, which we ordinarily speak of as tastes, are really due to smell.* The distinctions between tea and coffee, between all the various flavors that make the difference between candies and sugar, between various syrups and so on—all these are lost. That the absence of marked sensations during eating should lead to a relative neglect of such sensations is natural; Mr. E. is perhaps on this account less sensitive to other mouth sensations.

Mr. E.'s defect was observed by members of his family as soon as the sense of smell could be tested. He has no recollection of ever having smelled and his family agree that he never gave evidence of such sensation. It is certain that he could not smell when a very small boy. He gets no sensations from flowers, perceives no difference in the taste of his food when afflicted with a cold, and observes no distinctions in the various kinds of sweet things of which he is fond. He perceives no distinction between tea, coffee, and hot water flavored with milk and sugar and has come to take the latter as his every-day drink.

Mr. E.'s case is especially interesting because his mother has a similar defect. Mrs. E. however at one time possessed the sense of smell and distinctly remembers the sensations derived from odors and her use of odorous substances. She seems to have lost the sense when about 13 or 14 years of age. It is definitely established that she is the first and only one of her family or her husband's family to show this defect. She has two sons and two daughters besides Mr. E., all of whom are normal as regards the sense of smell. Some of the more typical experiments performed upon Mr. E. were also tried by Mrs. E. with strongly corroborative results. Ammonia alone had a marked effect; the same confusions were made by Mrs. E. as by her son. She is likewise deficient in distinguishing "by taste" between flavoring extracts and similar substances.

CLASSIFICATION TIME.

(With the assistance of GEORGE W. MOOREHOUSE, Fellow in Psychology, and MILDRED HARPER.)

An extremely frequent and important mental process consists in the reference of some item of knowledge to some familiar class. The assimilation of knowledge involves the appreciation of the relation of the new fact to the general body of acquired knowledge. In order to maintain in an orderly and accessible form our mental acquisitions it is necessary to view each item of information as belonging to such and such classes. Psychologists have variously analyzed this process; some express their views by picturing the mind as possessing a number of apperceptive instruments and using now one and now another of these according to the nature of the object to be assimilated, or, again, as a series of lanterns each of which has its own focus and field of illumination. However we may view the process it is clearly essential to the acquisition of knowledge, and it is strange that the study of the time-relations of this process has been hitherto so largely overlooked. The present contribution considers the time of a special form of such classification.

As a distinctive and readily studied form of such reaction we selected the reference of a common word to its grammatical class. We further limited the problem by selecting the following ten nouns, ten verbs and ten adjectives and confining our reactions to calling the proper part of speech to which one of these words belonged: house, cat, book, ship, ant, sun, lake, doll, man, girl; push, have, cut, mix, go, die, look, sit, jump, touch; tough, wet, good, blue, low, bad, high, thin, hot, black.

These words were chosen as familiar, distinctive and monosylabic representatives of their classes. The full list of words which he might be called upon to classify was always read to the subject before each kind of experiment. We reacted to a spoken word by a spoken word. The apparatus by which this was accomplished consisted of a bit of wood held between the teeth connected with one arm of a lever the other arm of which bore a metallic point for electrical contact. A spring connected with the lever tended to pull the bit from between the teeth, and according to the adjustment to make or break an electric circuit. Both subject and observer used an instument of this kind, the instruments being so connected with the chronoscdpe that the release of the bit from the mouth of the observer started, and a similar action from the subject stopped it. The necessary act of separating the teeth that accompanies articulation is here taken as the point of measurement. The apparatus is fairly satisfactry, and so long as the results are used mainly for relative purposes the error involved in its use may be neglected. A perfect apparatus whereby the utterance of a word will start or stop a chronoscope is still a desideratum.

The entire process may be viewed as consisting of the following steps: (1) the hearing of the sound uttered, (2) the recognition of the word, (3) the reference of the word to its class, (4) the summoning of the term describing that class, (5) the muscular innervation accompanying the utterance of the term. In order to determine the time of the purely mental process involved in expressing the fact that a certain one of ten words is a noun, or verb, or adjective, it was necessary to measure separately the time of the mechanical steps. The simple reaction involved evidently consists of steps (1) and (5). This time for each of the three subjects we found to be 190^σ, 195^σ and 199^σ respectively. It is naturally somewhat long for a simple reaction because the muscular contractions by which it is signalled that the impression has been received are complicated, and because the moment at which the chronoscope starts may slightly precede that at which the sound-wave reaches the subject's tympanum. In all these simple reactions both observer and subject used what seemed to be the easiest vocal utterance; it consisted of a violent expiration, the result resembling the sound *eh*.

We further need in order to measure step (3) in which we are particularly interested a process involving steps (2) and (4) as well as the simple reaction. It seemed impossible to devise any simple process of the kind, but the process of repeating a word sufficiently approximates it for our present purpose. This process clearly involves in addition to the simple reaction, the recognition of a word and the summoning and utterance of a word. The only question would be whether the summoning of a term denoting a grammatical class is of equal difficulty with the repetition of a recognized word, but as both are very familiar and somewhat mechanical processes, their time relations can hardly be very different. The repetition time for the three subjects was as follows: 367^{σ}, 280^{σ}, 333^{σ}.

The experiments in which words were referred to grammatical classes were of the following kinds: (1) the subject was to tell whether the word was a noun or verb; (2) the same distinction regarding nouns and adjectives; (3) the same distinction regarding verbs and adjectives; (4) the same distinction regarding nouns, verbs and adjectives. Experiments are grouped in sets of twenty each. In fact from 22 to 25 observations were taken and those most divergent from the average of all were discarded until 20 were left. A new average of these 20 was entered. The following table gives for each of the three subjects the average time of the several reactions together with the number of sets of which it is the average.

Subject.	Simple.	Repeat.	'Noun-Verb.'	'Noun-Adj.'	'Verb-Adj.'	Average of 'N-V,' 'N-A,' and 'V-A.'	'Noun-Verb-Adj.'	Mental Time.
J. J.	190 (15)	367 (10)	599 (18)	595 (10)	593 (10)	596	667 (11)	71
G. W. M.	195 (17)	280 (17)	628 (13)	597 (10)	679 (10)	635	678 (9)	43
M. L. H.	199 (10)	333 (11)	612 (15)	550 (10)	568 (10)	577	589 (10)	12
Average.	195	327	613	581	613	602	645	42

Combining the results of the three observers we obtain as the result the fact that with a reaction time of 195^{σ}, and a repetition time of 327^{σ}, it takes 603^{σ} to determine whether one of a set of words belongs to one or the other of two grammatical classes (the mental portion of this process consuming 276^{σ}), and that it takes 645^{σ} to refer a word to one of three grammatical classes.

It hardly seemed worth while to calculate the mean variation of these observations, but to satisfy ourselves regarding the regularity of the results we calculated it for the three most typical sets under each kind of reaction. Expressing the mean variation for these three sets as a percentage of the general average time for the kind of reaction, we obtain the following table.

Subject.	Simple.	Repeat.	'Noun-Verb.'	'Noun-Adj.'	'Verb-Adj.'	Average of 'N-V,' 'N-A' and 'V-A.'	'Noun-Verb-Adj.'
J. J.	8.0	8.4	11.0	10.3	9.9	10.5	9.5
G. W. M.	16.5	13.2	11.8	13.4	10.9	12.0	13.4
M. L. H.	20.9	9.7	8.2	5.6	7.6	7.0	8.3
Aver.	15.0	10.3	10.3	9.9	9.6	10.0	10.4

This table indicates a very fair degree of regularity with the exception that, markedly in the case of M. L. H., and to some extent in the case of G. W. M., the variation for the simple reaction is large. This is clearly due to the fact that experimentation began with the simple reaction alone so that this variation indicates absence of practice in reaction work.

The results are, however, effected by inequalities of practice. This is particularly true of the time for 'Noun-Verb-Adjective' distinctions which observations were made last and were therefore most benefitted by the practice gained in the former distinctions. It is probable, therefore, that the difference in time, 42^{σ} between the two processes is too small. This appears more clearly in considering the results for each subject. For J. J., who began with most practice in this kind of observation and whose time for the three classes of distinction, 'Noun-Verb,' 'Noun-Adjective' and 'Verb-Adjective,' show greatest constancy, the difference in question is largest, 71^{σ}. For G. W. M., who began with some practice in reaction experiments the difference is intermediate, 43^{σ}, and would be greater were it not for the special and temporary difficulty he encountered in distinguishing verbs from adjectives, the difference between the average of the 'Noun-Verb' and 'Noun-Adjective' and the 'Noun-Verb-Adjective' being likewise 71^{σ}. While for M. L. H., who began with no practice and showed steadily decreasing time for each successive kind of reaction attempted, the difference in question is but 12^{σ}. It is probable then that the time for J. J., 71σ, is a more typical result than the general average, 42σ.

The relative difficulty of the three pairs of distinctions, ‘Noun-Verb,’ ‘Noun-Adjective’ and ‘Verb-Adjective’ probably varies with different individuals; in the present study it is also affected by differences of practice; on the whole, however, our results favor the view that the three are of practically equal difficulty.

The increase in time in passing from two distinctions to three is an interesting illustration of the effect of the mental attitude on reaction times. The process involved is the same in both cases, to decide, for example, that *man* is a *noun*, but this decision requires more time when the word in question may belong to one of three grammatical classes than when it may belong to one of only two. Our results indicate that all of these processes are quite complicated and that their time-relations depend upon the accessibility of very familiar items of knowledge.

Regarding possible differences between the several words, they may vary with individuals; extended results would be needed to clearly show their existence. It is interesting, however, to observe that taking the average reaction of each word in all the three kinds in which it occurs we find among nouns “ship” was most quickly classified by all three subjects; another easy word was “man”; especially difficult nouns that were “doll,’ “ant” and “cat”; among verbs “sit,” “jump” and “go” were relatively easy, “have” and “cut” relatively difficult; among the adjectives “good” was particularly easy, “wet” and “blue” fairly so, “high” particularly difficult, “bad” and “hot” fairly so. It should be noted, too, that this difficulty may in part be due to difficulties of recognition and pronunciation.

FINDING-TIME.

(With the assistance of WINIFRED SERCOMBE and LUCY M. CHURCHILL, [MRS. FRANK T. BALDWIN].)

We have employed the term “finding-time” to denote the time occupied in the process of finding a given object within a given field; we recognize with what different facility and rapidity different persons perform such tasks, and even in the same individual the time is subject to variation. We have all experienced the difficulty of finding an object even when it is plainly in sight, and have wondered at the long time necessary to find a quotation in a volume and the like. In this process we carry with us a mental picture of the object sought and we react when the subjective corresponds to the objective picture. The ability to recognize one o
of objects as the one desired is certainly a n

may perhaps be a convenient test of mental alertness. It is this process that we desired to study and to measure. The difficulty of finding an object varies with several factors, the most important of which may be thus summarized: (1) the number of objects amongst which one is sought; (2) the nature of the object; (3) the minuteness or complexity of the differences by which the one object is distinguished from the others; (4) the degree of probability (which may amount to certainty) that the object sought is within the given area.

In our study the objects sought were the letters of the alphabet; the method of finding them was as follows: The letters (plain capitals about 4 dioptrics or in the average 6.5 mm. square and very closely conforming to the Snellen types) were gummed on a card which was in turn fastened on a block, and were seen through square openings in a black screen. These openings, 25 in number, were 11 mm. square and were each separated by 19.5 mm. above and below and to each side from the neighboring opening; this screen was laid on a glass plate mounted in a square frame that slipped over the block and (inside) was about 15 mm. larger each way than the block. The block contained four alphabets distributed by a chance arrangement, and according as the frame was moved to the upper left hand, the upper right hand, the lower right hand, or the lower left hand corner, one or another of these alphabets was seen through the openings in the screen. The arrangement may be made clear by reference to the letters below. Here each different kind of type represents an alphabet and it will be clear from this how a simple movement was sufficient to bring to view through the openings in the screen another alphabet. In the original all the letters are of course alike, and distributed by a chance arrangement. Connected with the frame by means of two iron uprights was

C S X O R L T M O T
F I R C O G A R C H
I J P A·H B B C F K
G S U N S L I A E J
Z G N V K W S E W Z
M F Z B W P B D F K
J R E D V F D H Y N
D Z Y U J E V X H V
G Y L X U I M F A U
L Y X M T W K T N O

a head piece similar to that of a stereoscope against which the subject rested his head and through two openings in which he viewed the letters. Across these openings is a hard-rubber flap which may be quickly withdrawn by bringing into action a strong spring. As this flap opens it closes an electric circuit and thus starts the chronoscope.[1]

An observation was conducted as follows: the frame is set for a certain alphabet; the operator announces the letter to be found (this also serves as a signal) and shortly thereafter he pulls a cord releasing the spring and allowing the subject a view of the letters. As soon as the desired letter is seen the subject presses a key and stops the chronoscope. To test whether the subject knows where the letter is situated he keeps a record of each answer. The positions were indicated by lettering the double rows A, B, C, D, E, and the columns 1, 2, 3, 4, 5, so that A1 would indicate the upper left-hand corner, D5 the lower right hand corner and C3 the centre letter. In the first experiments 25 letters were thus shown (Q was omitted), but this could be reduced to a four-square (16 letters) by covering over either the row A or E together with either column 1 or 5. Throughout the experiments except when distinctly stated otherwise, the subject was assured that the letter sought was present.

The following table represents our average results for the three observers separately and together.

	Finding one of 25 letters.	Placing one of 25 letters.	Finding one of 16 letters.	Placing one of 16 letters.	Finding one of 25 letters, with 9 letters absent.			Finding two of 25 letters.
					Average.	Absent.	Present.	
J. J.	$(^{20})$582	$(^{13})$309	$(^{10})$413	$(^{7})$316	$(^{10})$915	1085	817	$(^{7})$1445
W. S.	$(^{16})$485	$(^{11})$210	$(^{10})$302	$(^{10})$175	$(^{10})$649	722	610	——
L. C.	$(^{13})$640	$(^{10})$355	$(^{10})$428	$(^{10})$288	$(^{10})$761	1048	651	$(^{12})$1836
Aver.	569	291	381	260	775	952	693	1640

The numbers in parentheses indicate the number of sets of 20 observations from which each average was derived; the other numbers represent the average times in σ=.001 second. We see that it took on the average 569^{σ} to find one of 25 letters and 381^{σ} to find one of 16 letters. The process is thus quite complicated and is very difficult at first, the stage of initial practice being quite marked and the first few sets yielding very long times. Considerable of this time is consumed in the process of accommodating the eyes to the plane of the letters and bringing them clearly into view. We considered that this time would be measured by measuring the time needed to see what letter occupied a certain position amongst the twenty-five. Instead of calling a letter and reacting when its position was seen, a position was called, for example A1, C3, etc., and the subject reacted when the letter occupying that position was recognized. The subject here knows just where to look and, although this time includes the recognition of the letter as well, we should remember that it is probably fair to exclude this element from the time of finding letters, the finding time strictly applying only to the process of search. While therefore only an approximate elimination of the mechanical process is obtained by subtracting the "placing time" (as we shall call this latter step) from the "finding-time," yet this difference very fairly represents the distinctive part of the finding process and is remarkably alike in the three subjects, 273^{σ}, 275^{σ} and 285^{σ}.

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[1] The essential features of this apparatus as well as of the problem investigated were suggested by Prof. G. Stanley Hall and were elaborated in conjunction with him at Johns Hopkins University some years ago.

The effect of the number of objects amongst which one is to be sought, and of the larger field is illustrated in the difference of time between finding one out of 16 and one out of 25 letters; this is on the average 188^{σ} and in the individuals 169^{σ}, 183^{σ} and 212^{σ}. The ratio of the times to find one out of 25 and one out of 16 letters thus increases in the proportion of 1.55 to 1 which is just the ratio of 25 to 16.

In "placing" a letter, that is, in recognizing what letter occupies a certain position, it is obvious that the time should be little, if at all, affected by the number of places, and the slight difference between the values found for placing one of 16 or one of 25 letters, 260^{σ}, and 291^{σ} is probably due to the fact that the former sets represent a more advanced stage of practice than the latter.

The next variation presents an interesting difference; 16 letters are present and, as before, these change with every observation, but instead of calling only for those letters that are present, any one of the 25 letters may be called for, and if not present the subject reacts as soon as he is convinced of its absence. The average result of all the experiments performed in this manner is 775^{σ}; this, however, is not as significant as the result we obtain by considering separately those cases in which the letter to be found was present and those cases in which it was absent. That it should take longer to go through a series of 16 letters and determine that a certain one is absent, than to determine its presence, is to be expected; the difference is certainly great whether we compare it with the finding time of one of 25 letters or more properly with the finding time of one of 16 letters. It takes 571σ longer, or 2½ times as long, to determine that a given letter is not among a group of 16 than to find it if it is present. But while it takes 381σ to find one of 16 letters when the subject knows it is there, it takes 693σ to perform precisely the same process when there is a chance (strictly when there are 36 chances in a 100) that the letter he is seeking may be absent. This result most strikingly illustrates the effect of the fore-knowledge of the subject upon the time of mental processes; the apparently simple process of comparing an objective with a subjective image varies its character according to the underlying connection by which the process is accompanied. This result, too, appears in the fact that, while in finding letters all of which are known to be present, an error is exceedingly rare when the letters may be absent. Errors are quite numerous and consist in declaring a letter that is present to be absent.

In certain processes it is relatively easier and quicker to do two things together than to perform them separately; this being due to an overlapping of the mental processes. There is a division of the attention among the several mental tasks so that the time needed for the whole is considerably less than the sum of the times needed to do each separately. In other cases the attempt to perform processes together seems to result in a mutual inhibition or confusion and a loss of time and energy. As a small contribution to an investigation of this problem we determined in two subjects how long it takes to find two letters among 25 and to note their positions. The two letters were announced beforehand and as soon as both were found the subject reacted. This proved to be a very difficult and often confusing process; it took on the average 1640σ which (for the two persons under consideration) is 418σ longer than twice the time needed to find one letter. This may serve as an index of the loss of energy in attempting to have two processes before the mind simultaneously.

While our results are not sufficiently numerous or free from great variation to warrant detailed inferences, yet there are two such questions of detail the importance of which justifies even the mention of the imperfect information we are able to give. The first relates to the difference in ease in recognizing the various letters. That such differences occur has been shown by more suitable methods. Our results show considerable variation; for one subject the range is from

393σ for W to 557σ for T; for another from 487σ for S to 719σ for L. On the whole the three letters most quickly found were S, O, and W; and the four least quickly found L, J, H and T. If we ventured to divide the alphabet into three groups of easy, medium and difficult letters, our lists would read: 1. S, O, W, N, D, C, E, I; 2. X, B, Z, G, M, Y, A, R, B; 3. K, U, F, V, T, H, J, L. It must be remembered however that no great weight is to be placed in this detailed result. The second question involves the query whether the letters nearer the centre of the block are more readily found than those away from the centre. Our results are unfortunately not recorded in such form as to readily allow of the determination of this point; but we compared the times for all the letters found in positions B3, D3, C2 and C4, that is, in a diamond about the central letter C3, with those for finding the four positions furthest removed from the centre, A1, A5, E1, E5. Our result showed a slight excess of time for finding the peripheral letters, an excess too slight perhaps to be recorded were it not for its constancy in all three individuals.

This first attempt to gain a deeper insight into the mental process of finding certainly leaves untouched the larger number of important and suggestive queries attached to it, and yet the results obtained are sufficiently clear and consistent to justify the promise of future investigation.

SOME ANTHROPOMETRIC AND PSYCHOLOGIC TESTS ON COLLEGE STUDENTS.—A PRELIMINARY SURVEY.

(With the assistance of GEORGE W. MOREHOUSE, Fellow in Psychology.)

During the fall of 1890 it was decided to ask the students in the general class in Psychology to lend themselves to series of physical and psychological tests with a view of interesting the students in such tests as well as acquiring a body of statistical material which when sufficiently extended and properly compared with other statistics might prove of considerable value.

The experiments were not extensive in character but they served to bring out the difficulties in this line of work, and the publication of the present fragmentary results[1] is ventured in the hopes of furthering similar observations elsewhere.

The tables given below require more or less explanation and comment. The physical measurements of the men are in the main those regarded as most important by Mr. Galton, and were made with the intention of correlating mental with physical characteristics. The apparatus employed was very simple and hardly needs description. The dynamometer is of the Feré pattern, made by Cullin, Paris. Similar measurements for the women were obtained through the courtesy of Miss Ballard, in charge of the Ladies' Gymnasium, but were too few in number to warrant tabulation.

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[1] Simple and few as the tests were they required about 50 minutes for each student. If the tests could be arranged so that several persons might be tested together without interference a great saving of time would result.

In four cases the measurements made by Mr. Galton upon miscellaneous Englishmen are exactly repeated upon these college students, and the results indicate in so far as such few results can indicate, a superiority in favor of the college students.

The sensibility tests were selected to quickly yield a few typical results. Like all such observations the chief difficulty lies in the fact that the subjects are not used to accurately observing their sensations, so that a relatively brief practice would in many cases alter the result. The æsthesiometer employed was that described in this JOURNAL (Vol. I, p. 552). It appears that the distance at which two points could be felt as two on the back of the hand was 16.4 mm. and on the finger-tip 1.63 mm.; the former result being strikingly small as compared with Weber's tables.

The sensitiveness of the palm was tested by determining the minimum height from which the fall of a bit of card-board could be perceived. These bits of card-board weighed .9 mgr. and were cut in rectangles of 1 by 2 mm. from a sheet of millimeter paper pasted upon the card-board.

The apparatus used for testing the pressure sense was a modification of Fairbank's post-office balance in which the weights were placed upon the scale pan, thus exerting an upward pressure upon the finger resting upon a cushioned plate at the end of the beam. A comfortable and firm position was secured and an attachment provided by which fatigue was prevented. Two-sevenths of the weight on the scale pan acted upon the finger. The table records that additional weight (to the nearest 25 gr.) which could be correctly distinguished about 3 or 4 times from an initial weight of 500 gr. But few observations were taken and the result is only approximate. The general result is that a difference of about $\frac{1}{6}$ or $\frac{1}{7}$ of the initial weight may be correctly appreciated.

We also attempted to measure sensitiveness to pain. For this purpose we used a light hammer (weight 98.3 gr.) pivoted at a point 200 mm. from the center of its iron head, and allowed it to fall on the tip of the fore-finger of each hand. The back of the hand as well as the finger struck was supported. The table records the minimum number of degrees through which the hammer must fall in order to cause a painful sensation. While this is naturally not a clearly defined point, still its constancy was surprising. The left hand appears to be more sensitive than the right. As few falls of the hammer as possible should be used in this test as the skin rapidly fatigues.

We take up next a description of the tests of vision. The printed page was first placed beyond the subject's vision, then gradually moved toward him along a sliding scale until he could just read it. The column of the table gives the distance at which, with the maximum strain, the page could be read. The size of the type is that in which this article is printed. The same page was then held as close to the eye as possible and yet have the subjet able to read it. We next record the smallest size of print (in dioptrics) that could be read at 25 feet.

For the next test we prepared a large white disc with small black sectors ranging from 1° to 15° and proceeding by half-degrees up to 5°. When this was rotated there appeared a series of concentric rings of various light shades of gray, each ring being 10 mm. wide and separated by 5 mm. from its neighbors. The subject counted as many concentric rings as he could see, and the result was then read off in degrees.

The acuteness of vision was tested in several ways, (A), by finding the distance at which a series of black lines 1 mm. wide and separated by spaces of 1 mm. could be recognized and the spaces between the lines clearly discerned, (B), by a similar determination with a checkerboard pattern, both black and white squares, being 4 mm. square, and (C) by the distance at which either 7 or 8, 11 or 12 and 15 or 16 dots 2 mm. in diameter and irregularly arranged in a rectangle of 25 x 40 mm., could be counted. The results are recorded in inches.

Our next test related to color and we attempted at the same time to detect any color defects, and to get some measurement of the rapidity and accuracy of color distinctions. Each student was required to match as rapidly as possible 30 colored ovals of a Magnus-Jeffries Color Chart (as published by Prang). We also noted irregularities in matching. The average time shows about six seconds for each color.

The strength of vision we tested by noting the smallest size of letter readable at 25 ft. through one and through two thicknesses of common cheese-cloth. No student could see the letters at all, up to 50 dioptrics through three thicknesses. The result is recorded in dioptrics.[1]

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[1] The only test for hearing that we attempted was to determine from what height a shot weighing 10 mgmm. must be dropped upon a glass plate to have the sound heard by the subject at a distance of 25 ft. The

We also made a few tests of the rapidity of movement. This was done by arranging two keys so that the closure of the one would start a Hipp Chronoscope and of the other would stop it. The distance between the keys was in the one case 38 inches and in the other case 3 inches. With the keys 38 inches apart the subject was first told to touch them in succession, not as fast as possible but at any rate which seemed natural to him. He next made a movement of the same extent, as well as one of 3 inches, as fast as possible. This was done separately for the right and left hands, and the average time of about 5 movements is recorded in the table. The movement must be somewhat accurate in order that the key shall be struck at each end. The results for the maximum movements enables us to determine that the movement alone was at the rate of about 8 feet per second.

It had been our intention to meet each student a second time and with this intention we inaugurated a series of tests of sense-judgment, only a very small portion of which was completed, namely those relating to pressure and one relating to the space sense of the skin. The subject was first required to pour as much shot in the palm of his right hand as he thought would weight an ounce. The average weight of the shot thus estimated to weigh an ounce was 37 gm., or an exaggeration of 13% (men 47 gm., an exaggeration 65%; women 22 gm., an underestimation of 21%). He was next asked to pour as much shot into a box (3¼x3¼x4 in. made of ⅛ in. pine) as he thought necessary to have shot and box weigh one ounce. In this case the average result was 97 gm. or an exaggeration of 242% (men 100 gm., exaggeration 252%; women 92.5 gm., exaggeration 226%). The illusion involved in this test is the well known fact that a stimulus spread over a larger area seems much less intense than a like stimulus confined to a more limited area. The result, in the two cases given above, measures the degree of the illusion. He next repeated the operation with the intention of making the box and shot weigh one pound. The average result was 548 gm. an exaggeration of 28% (men 605 gm., an exaggeration of 34%; women 463, an exaggeration of 2%). We find here a smaller percentage of exaggeration than in case of the ounce. He was then given the box which he regarded as one pound and irrespective of its actual weight was asked to put enough shot into another box to make it

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average result 27.8 mm. is inaccurate owing to the impossibily of securing absolute and constant quiet. It is interesting to note that the hearing of the women was more acute than that of the men, the results being 17 and 35 mm. respectively.

weigh double the first. The average result was 879 gr. or an underestimation of 20% (men 940 gr., underestimation 23%; women 789 gr., underestimation 15%).

The space-test consisted simply in spreading the points of the æsthesiometer on the back of the subject's hand until he regarded the distance between the points to be one inch. The average result was 30.6 mm., an exaggeration of 20% (men 31 mm., exaggeration 22%; women 30 mm., exaggeration 18%).

It is interesting to note that in all these tests of sense-judgment the women are more correct than the men.

In addition to this a few tests on bilateral symmetry of motion were made upon 17 of the lady students. They were asked to move the fore-fingers of the two hands outward from a common point along horizontal bars of a wooden cross the intention being to move the two arms to an equal distance. The movements were first made with the fingers at all points resting on the bar and were further subdivided into fast movements and slow movements, and again into large movements and small movements. All these variations were also gone through with for movements in which the fingers were lifted up into the air and brought down upon the bar at the end of the movement, (free movements). The table shows the result from each of these variations. It appears that, in each case, the right hand makes the larger movement, the excess on the average amounting to 15.5 mm. Regarding the extent of the excess of the *preferred* hand it is necessary to note that one student is markedly left-handed and another nearly ambidextrous. In both these cases the left hand makes the larger excursions and thus the average excess of the *preferred* hand becomes 16.7 mm. or $\frac{2}{3}$ of an inch.

It appears that the most influential of the distinctions made is that between the guided and the free movements, the average excess of the preferred hand in the case of the guided movements being 10.1 mm. and in free movements 23.4 mm. The size of the movement is of some influence upon this excess, it being on the average 21.3 mm. for the large movements and 12.1 mm. for small movements. In slow movement the excess of the preferred hand is more marked than in fast movements, being 19.9 mm. in the former and 13.5 in the latter. Individuals show considerable difference in the amount of this excess of the preferred hand, the average excess for the 17 different individuals being as follows: 54.3, 30.7, 30.1, 25.1, 22.6, 20.6, 17.6, 17.0, 12.8, 12.6, 10.9, 10.7 (left), 9.9, 9.0, 8.0, 8.0 (left), and 3.9 mm.

In addition to the measurements given above we placed before them a series of miscellaneous questions in regard to personal and family characteristics. From the answers to these questions we collect the following data: the average age was 21 yrs. 11 mo. (31 men 22 yrs. 4 mo.; 22 women 21 yrs. 4 mo.) Of the 53 students 45 were born in Wisconsin, 7 in adjoining states while 1 is of foreign birth. Regarding the birth-place of the parents, in 29 cases it is in foreign lands, 23 in New England States, Vermont predominating, 32 in Middle States (N. Y. 28, Penn. 4), 21 from Western States.

The occupation of the father was noted with the following result: 15 merchants and manufacturers, 10 farmers, 13 professional men, 5 officials, 4 mechanics, 5 bankers and real-estate dealers.

When asked to state whether they regarded their health as "excellent," "good," "middling" or "poor," 20 (14 men and 6 women) pronounced it "excellent," 28 (13 men and 15 women) "good," 4 (3 men and 1 women) "middling" and 1 "poor." When questioned as to the existence of headaches or other chronic complaints 30 (16 men and 14 women) declared themselves free from all such, 13 (9 men and 4 women) were troubled with headache and 7 with other complaints.

46 of 52 students (27 men and 19 women) called their sleep "regular" and the rest "irregular," and 33 of 46 students (23 men and 10 women) spoke of their sleep as "sound," and the rest as "light." The average duration of sleep was just 8 hours.

It will be interesting to compare, as far as possible, the records of the men with those of the women. The general result regarding dermal sensations is that women have finer sensibility than men. This is true for each one of the tests made, but the differences are comparatively slight, except for the absolute sensitiveness of the palm and the sensitiveness to pain. The greater sensitiveness in women in both of these cases indicates freedom from rough usage.

As regards vision the differences on the whole are so small as to prove no superiority in the one case or in the other. To this there is but one exception and that is in the accuracy and rapidity of color perception in which the women are clearly better than the men.

Finally regarding the rate of movement, the normal movements, that is those adopted when no special dir
given, are quicker in women than men while th
movements, particularly in case of the longer mo
faster in men. All these differences are con
to well recognized differences in the two sex
use and development of the different senses

TABLE I.

Physical Measurements (of 31 Men, in mm.)

Height Standing.[1]	Height Sitting, from Seat of Chair.	Span of Arms.	Chest Girth.	Head Girth.	Strength of Squeeze.[2]
1748	926	1813	910	575	41.25

TABLE II.

Sensation and Movement.

Dermal Sensations.

Two Points Felt as Two.						Sensitiveness of Palm.			Pressure Sense.			Sensitiveness to Pain.					
Back of Hand.			Tip of Forefinger.									Right Hand.			Left Hand.		
T.[3]	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
16.4	17.5	15.0	1.63	1.71	1.52	44.0	58.2	21.9	82.5	83.7	80.7	26.7	33.9	16.6	19.3	22.7	14.8
(52)[4]	(30)	(22)	(54)	(32)	(22)	(49)	(27)	(22)	(53)	(31)	(22)	(53)	(31)	(22)	(52)	(30)	(22)

Sight (53 Students; 31 men, 22 women).

Distance at which print can be read.			Near point for print.			Smallest type visible at 25 ft.			Differentiation of white from gray.			Time for sorting 30 colors.			Distance at which lines can be recognized.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
52.9	53.5	52.1	2.5	2.4	2.7	8.3	9.4	6.7	2.62°	2.74°	2.42°	177″	212″	130″	108	117	97

Sight, continued, (53 students; 31 men, 22 women).

Distance at which dots can be counted.									Distance at which checker-board pattern can be recognized.			Letter visible through cloth.					
7 or 8.			11 or 12.			15 or 16.						1 thickness.			2 thicknesses.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
157	155	159	141	141	140	101	108	91	122	121	124	22.0	24.7	19.0	43.5	45.0	42.0

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[1] The height of heel (average 21.2 mm.) has been subtracted from full height.

[2] This measurement was taken upon only 16 men and is expressed in kilograms.

[3] M is the result for the men, W that for women, T the average of both.

[4] The figures in parentheses give the number of persons tested.

Rate of Movement (45 students; 28 men, 17 women).

Movement through 38 inches.												Movement through 3 in.					
Normal.						Maximum.						Maximum.					
Right Hand.			Left Hand.			Right Hand.			Left Hand.			Right Hand.			Left Hand.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
1000[1]	1070	885	908	964	817	542	506	601	527	498	574	181	181	181	185	172	205

[1] These numbers indicate $\sigma = .001$ sec.

TABLE III.

Symmetry Movements.

Guided.								Free.							
Fast.				Slow.				Fast.				Slow.			
Large.		Small.		Large.		Small.		Large.		Small.		Large.		Small.	
R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.
496	494	179	170	493	505	190	177	505	497	199	185	510	480	180	168

Addition to Literature Notices under article on Zöllner's Illusion.

MÜLLER-LYER (Du Bois Reymond's Archiv. Supp. Band 1889) gives a brief but valuable account of a variety of optical illusions of judgment. He clearly demonstrates the influence of angles, of positions of figures, and the like upon their apparent size. His explanation of the illusions refers them to the tendency of considering surrounding and suggested areas in the judgment of lines and areas. He also mentions the effect of the angle in Zöllner's illusion, but does not enlarge upon its relation to the other illusions. The article, while comprehensive and original, does not add materially to the explanation of the illusion[1].

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[1] The illusions of contrast in our article are described in Müller-Lyer's article. While I had read this article in 1889, I had entirely forgotten about it in the present investigation and worked out the present figures, which I had not seen before (they are not figured in Müller-Lyer's article but only i[illegible] ndependently. Dr. Sanford has drawn the figures described by [illegible] ttention was again called to this figure and article af[illegible] J.

Corrections to "Studies from the Laboratory of Experimental Psychology of the University of Wisconsin." AM. JOURNAL OF PSYCHOLOGY. Vol. IV., No. 2.

On page 199 insert the following table, accidentally omitted:

RANGE OF WORDS.	J. J.	F. W.	MOTOR.		SENSORY.	
	σ.	σ.	J. J.	F. W.	J. J.	F. W.
Any word whatever	269	267	266	262	272	272
One of 100 verbs	260	265	253	263	265	267
One of 50 animals	250	262	250	256	250	268
One of 20 names	238	249	233	216	243	252
One of 20 letters	238	243	237	233	239	252
One of 10 French words . . .	245	251	246	249	244	253
One of 10 numbers	229	233	227	232	231	234
Simple Reaction Time	177	187	174	184	181	191

The pages in "Accessory Apparatus for Accurate Time-Measurements" belong to the study of "The Effect of Foreknowledge upon Repetition-Times," and the "Note upon Apparatus and Method" (p. 200) is a part of the former.

The "Note A—On the Timing of Rotating Discs," and the "Note on a device for color mixing "(p. 211) belong to the study of "A Novel Optical Illusion," and should be credited to Mr. Moorehouse. In the cut (p. 210) the letters B P on the right hand side should be B′ P′.

THE PSYCHOLOGICAL FOUNDATION OF NATURAL REALISM.

By Alexander Fraser, A. B.

The ordinary, common-sense man lives and thinks on the assumption of two fundamentally distinct and frequently conflicting worlds, the world of ideas and the world of things. The distinguishing characteristics of these two worlds are to him,—to put it in a word,—that the former may be and often is illusory, and that the latter must be and always is real, always the same permanent, unchangeable world. Throughout the greater part of his life the two coincide and present to him the appearance of only one, but occasionally there come critical moments at which they must part company and leave as a result of their conflict and separation a firm conviction of a real dualism; the world falls apart into two general classes, of which one must be real and the other may not. It is at such periods in the uncritical common-sense life, that it is easiest to observe the primordial germ of dualism, and the special psychological foundation of that belief in a real external world which is the presupposition of all practical life and the guiding-star of all realistic systems of philosophy. In the case of the unreflective but practical thinker, the question, what do you mean by a real world? is answered openly and without bias. The most general statement of his answer is: It is a world that we can *touch*. What is necessary according to him, in order to constitute the essential features of a real world, is that it be in some way or other *tangible*. What he means by the reality of an object seen in the distance is the belief that if he were beside it he could touch it; if upon approaching it he found that he could not feel anything, he would say that it was not real but illusory. What he means by an illusion, ghost or phantom, is, in an ultimate analysis, something which is in its very nature intangible. He can be persuaded that the object he *sees* before him is illusory; but if he is allowed to stretch forth his hand and can touch it and feel it there, the last remnant of doubt as to its real existence will have fled. Or conversely, he can be persuaded that the ghost or phantom which he sees is really there, but if he puts out his hand and feels it not, then he is firmly convinced of the illusion. Practical life is full of illustrations of this truth, and I think that without making any further explanation, we can safely carry with us for future use the general conclusion that the final and most conclusive test of reality for the common-sense man is "touch."

But this truth can be seen in a much deeper and more critical sense. Let the common-sense man begin to philosophize. Let him become acquainted with Berkeley's theory of matter. He is told that this real world of his in which he has had all faith ever since his life began, is a monstrous illusion; that there is only one world and that that is not what he used to call his real world but his ideal world; that he is to be deprived of not one of his old facts, but that all these facts are of the same type and this type is the type of his ideal world. He is at once fascinated by the novelty of the doctrine. At first he will have an irresistible objection to it on the ground of his old appeal to reality—he will invariably reply, there is more than the *idea* of the world there, for I can *touch* it. But he is asked to *reflect*, to look within and to say what he really means by "touching an object," he is asked to describe, to give a definite expression to the content of consciousness which corresponds to this fact of touching; once more he begins to see the truth of idealism, and his stubborn realistic notions begin to fade and grow dim. He finds that all he means by "touching an object" is the *idea* that his hand (another idea) stands in a certain relation to an object, which is itself only an idea. Everything he attempts to describe or express must first be translated into this language of idealism. It is all very well for men to live uncritically and to *believe* in an external material world; it is all very well to say that we can touch it, but the true and ultimate test now is not "touch" but "expression." Describe the content of your consciousness. Try to express what you mean by matter, try to define it, and you will find it immediately dissolving into ideas. The whole belief in a material world has arisen from want of reflection, from want of the proper method for observing the truth of things. The way to get at the truth of things is not to believe what *is* here but to wait until the next moment and then look back and see what *was* there. At the moment when we touch an object we have an immediate belief in its real existence apart from our knowledge about it, but we must not have any faith in this belief—we must find the real truth about the object by reflecting on this belief and by trying to give it a definite expression. The arguments of the idealist are unanswerable, and thus the common-sense man becomes a convert.

But let this same man arise from his philosophic calm, and let him once more go out and assume the duties of practical life; at the first stone he kicks, away goes Berkeley's theory of matter; he is back in his real world again. Idealism is very fascinating and all very true for a state of perfect calm, in which all the *active* senses are relaxed, but once out in the busy scenes of active life, its charms are gone, and all its terms appear hollow-sounding and meaningless. Underlying practical life there is a vast stretch of realistic intelligence which refuses to be expressed by the reflective method. It has no content in the imagination and consequently defies definite description. It seems to have been left without a language and without a written history. But nevertheless it has perhaps the highest claim to the name of intelligence since it realizes itself in immediate belief and practical life. Its outcome is not reflection but *action*. The real world we cannot and must not try to know by reflection, but we can and do know it by acting and living in it. How eagerly and yet how vainly do we search the whole vocabulary of language for words to express this great practical truth! How we have to fall back, as did the Scottish philosophers, on such generalities as "common sense," "belief," "intuition," which can be so easily ridiculed by the glib-tongued idealist whose rich inheritance is almost the whole vocabulary of thought! And what relief we feel in the reflection that we *are*, and *do*, more than we can know! Life and its fundamental beliefs are greater than knowledge; and the most fundamental belief, and the belief which stimulates and moulds all life, all evolution, all progress, is that belief which the ordinary man has in the existence of a real external world. It must be remembered that in this we are not dealing with any speculative form of realism. What we have been looking at is the simple experience of the naïve thinker. Our common-sense man has gone through the experience of idealism, and now he is back in his real world again. It is the same old world that once before he told us he could *touch*. It was by again allowing full play to the sense of touch that it was brought back to him with even deeper conviction than before. If we ask him now what is his criterion of reality he will reply not "definition" but "touch." He knows, and will admit, that there is something simple and uncritical about it, but yet he feels like crushing once for all our critical methods by telling us that there are more things in "touch" than were ever dreamt of in our philosophy. Such confessions from the ordinary unreflective life are of greatest importance inasmuch as they point out to us the history of the belief in an external world in its first stages, and in this indicate the fundamental basis of realism and the true method for its solution. And now that this fact has been pointed to, that realism at least in its first conscious forms, that at least the primary stages of a belief in external reality are most directly connected with, if not wholly founded upon, the sense of touch, we can go back still farther and read from the story of evolution how all this came about.

Now why is it that touch should be the organ of reality any more than any other sense? *A priori* there is no reason. The only way we can realize and appreciate the fact is by observing its history. Touch is the mother-sense. It is a result of the first division of labor in aminal life. The division of the protoplasmic mass into endosarc and ectosarc, or tactual surface is the first sign that marks its individuality. The tactual surface is the primordial boundary line between the ego and non-ego. It is most closely allied with the vital functions. In many of the lower forms, such as the Amœba, the absorbing surface and the contact surface are co-extensive; the vital functions and the tactual functions are almost one—the hand, mouth and intestine, are one and the same organ. As differentiation goes on, the tactual surface makes its special duty more marked. It becomes more and more confined to the business of mediating between the inner life and the outer world. If an outer world is to have any relation to, if it is to communicate in any way with, if it is to have any meaning for the inner life and vital functions, it must do so by means of tactual impressions. All the other senses, as Spencer has pointed out, are only modifications of the sense of contact. In their rudimentary stages the space penetrating senses are nothing more than anticipatory forms of touch. Their primary office is to serve touch. If they are to have any meaning for the life of the organism their impressions must be translated into impressions of touch. In the most highly developed forms the primary use of these anticipatory senses seems to have been forgotten, and they are admired for what they are in themselves. In man, for example, the visual faculty instead of remaining exclusively in the service of touch as a special scout between the inner life and outer reality has also become connected with the business of imagination, speculation and hypothesis. But in so far as any of these senses give any intelligence of an essentially real world, they must serve in their primary capacity and translate their impressions into the original impressions of contact. The organism cannot be affected in any important, in any *real* way, except by actual contact. All intercommunications and relations with an external world that are most closely connected with life are in their ultimate analysis relations of actual contact. Eating, breathing, locomotion, acquisition of food, struggles with and escape from enemies, all functions implied in the processes of life and evolution are functions which imply actual contact between the organism and its environment. Thus the sense of contact is that which is most closely allied with life on the inner side and with reality on the outer. It is the first and original *meaning* of reality. In the case of the other senses we may doubt and reason about the reality of the imformation received, but if we doubt the reality of contact we call in question the very standard by which we are enabled to doubt. And though in disease the sense of contact may deceive us and present to us illusions, yet the standard of sanity by which these phenomena are known to be illusions is the standard of contact.

Another fact which may be learned from the evolution of the sense of touch is the history of that *immediate belief* and *prompt reaction* which always accompanies it. In the case of the space-penetrating senses there is no absolute necessity for immediate belief in and prompt reaction to the information received. At the sight of the enemy in the distance it is not absolutely necessary that the animal should immediately take the proper measures for warding off the attack. It has plenty of time to stop and speculate as to whether it is a real enemy, admire its form, etc., and still have time left to make itself secure from danger. The anticipatory faculties are only the first warnings of approaching interests and may be and quite often are illusory and misleading. The characteristics of the reaction which follows must consequently be wavering, hesitation, delay, and speculation. But the case of contact is very different. By touch the final warning is given, and if it is not heeded and immediately reacted to, destruction or injury is sure to follow. There is no time for reflection, doubt, or speculation. It is the final signal and the animal which is not so constituted as to follow it with immediate belief in its reality and prompt reaction, will not survive in a real world. One of the fundamental conditions then, on which the sense of contact has survived as the special organ of a real external world, was that its outcome should consist in immediate belief and prompt reaction, and for this reason it is so to-day. At the beginning of the history of animal life, its sole function was to mediate directly between the inner life and the external world; this is its special function to-day in the latest stages of the history and in its most developed forms.

It may at first sight be thought childish to form all this real world of ours in all its fulness and vast complexity on such a simple, crude and seemingly unconscious thing as the sense of touch. Touch, however, is far from being crude. The mother sense, if it has not kept ahead, has at least kept abreast in development with the others. The influence of the sense of contact can be traced in all the highest forms of intelligence. Herbert Spencer says that touch is "more than any other sense associated with the advance of intelligence." He finds from the facts of evolution that "a highly-elaborated tactual apparatus comes to be the uniform accompaniment of superior intelligence." In support of this he supplies facts from each great division of the animal kingdom. The *Cephalopoda*, the most sagacious of the *Mollusca*, are especially distinguished in structure in having several arms by which they can grasp an object on all sides at the same time that they apply it to the mouth. Again the crabs which stand at the head of the sub-kingdom *Articulata*, bring their claws and foot-jaws simultaneously to bear on things they are manipulating. The parrot, which of all birds is admitted to be the most intellectual, differs most from its kindred in the development of its tactual organs. No other bird approaches it in the complexity of the tactual actions it performs and the tactual impressions it receives. Among mammals the *Unguiculata* or those having limbs terminating in separate digits are more intelligent than the *Ungulata* or hoofed animals. The feline and canine tribes stand psychologically higher than cattle, horses, sheep and deer. In the case of any marks of sagacity among hoofed animals, as in the horse, the lack of sensitive extremities is partly compensated by highly sensitive and mobile lips. The most remarkable and most conclusive instance of this connection between the growth of intelligence and development of the tactual organs is seen in the elephant, which is markedly distinguished from allied tribes both by its proboscis and by its great sagacity. The association between intelligence and tactual powers is brought out more conspicuously in this case by the fact that both are exceptional. Among the *Primates* the same association of development of intelligence with that of tactual appendages is distinctly marked both in contrasts between them and inferior animals, and between the different genera of themselves. The prehensile and manipulatory powers of the lower kinds are as inferior as their mental powers. In the case of the human being, Mr. Spencer maintains not only "that the tangible attributes of things have been rendered completely cognizable by the complex and versatile adjustments of the human hands, and that the accompanying manipulative powers have made possible those populous societies in which alone a wide intelligence can be evolved" but that even "the most far-reaching cognitions, and inferences the most remote from perception, have their roots in the definitely-combined impressions which the human hands can receive."

Again, it may be objected that the sense of contact as such is only a myth; that what we have been calling the mother-sense is only a name or hypothetical term introduced for the purpose of explaining the origin and differentiation of the other senses, and that there is no such thing as a definite and special sense of contact. The evidence from experimental psychology, as far as it has gone, goes to show that this objection is without good foundation. The sense of touch is perhaps of all senses the least explored, but the bulk of facts already obtained by experiment give evidence that apart from the variety of sensations generally grouped under the word "touch," *i. e.* the feelings of pain, exertion, fatigue, conæsthesia and muscle sense, there is a *special* sense of *contact*. Goldscheider by drawing a very fine point of metal over the skin discovered that at certain minute points a distinct and peculiar sensation of "pressure" was felt. This sensation, when the pressure is very light, is described as being lively and delicate and accompanied by the feeling of being tickled. When the pressure is increased, the character of the sensation changes and becomes as though a small, hard kernel were pressed upon the skin. Stimulation of the spaces between these spots does not produce the same characteristic sensation but rather a dull, indefinable, "contentless" sensation.[1] This special sense of pressure or of tactual hardness is incommensurable with any of the accompanying sensations. It cannot be explained by any possible combinations of any other senses such as the feeling of innervation, muscular resistance, etc., but it is in itself something unique and underived. Is it not the sense which alone gives us the essential nature of the primary qualities of matter? The feeling of muscular resistance has a meaning, but it is a very different meaning from that of resistance plus contact. Muscular resistance can never get beyond a muscular feeling—it can never mean hardness, solidity or those fundamental strata of matter which we call the primary qualities. Landry gives the case of a workman whose fingers and hands were insensible to all contact but in whom the sense of muscular activity was everywhere alert. His eyes were shut and a large object placed in his hand. He was quite aware of the muscular resistance but had not the slightest notion of an object, or that an object was in his hand; his only idea was that he could not close his hand, and he was astonished at the fact. Such facts as these, then, obtained by experiment, tend to show that there is a special sense of contact which is distinct from, and incommensurable with, the other senses; that this sense is the special organ for cognizing the primary qualities of the material world; and that consequently the mother-sense is not a myth or hypothetical name, but a real specific sense.

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[1] GOLDSCHEIDER, *Neue Thatsachen über die Hautsinnesnerven*, Du Bois-Reymond's Archiv, 1885, Suppl.-Band, 76.

Now that we have seen the historical foundation for the important part which the sense of touch plays in the practical knowledge of common sense, we can go still farther and trace its influence on the more technical forms of intelligence, science and philosophy. "All developed science," says Mr. Spencer, "dealing as it does with *measured* results, is lineally descended from that simplest kind of measurement achieved by placing side by side the bodies held in the hands. Our knowledge of the forces governing the Solar System is expressed in terms that are reducible, by an ultimate analysis, to equal units of linear extension, which were originally fixed by the direct apposition of natural objects. And the undeveloped sciences that have not yet passed the stage of qualitative prevision, depending for their advance, as they do, either on experiments requiring skillful manipulation or on observations implying dissection and other analogous procedures, could not have reached this stage in the absence of a highly developed manual dexterity." Science is not only mechanically dependent on the sense of touch but it is so in its very nature. The very world that science is striving to express is the world of contact. It never rests satisfied until it can define things in terms of the tangible. Contact is the presupposition of all scientific investigation. All psychological theories, for example, take for their starting point the conception of contact. The various empiricist theories of the development of the notion of space all begin with "contact." All theories concerning the processes involved in the functions of the various senses are attempts to reduce these processes to terms of contact. Sight is not explained by sight but by a hypothetical process instituted in order to allow actual contact between the retina and the object. In the same manner also are hearing and smell explained. Again all physical theories presuppose this same conception. All physical hypotheses about atoms, fluids, vibrations, etc., are just the outcome of this attempt to give expression to this fundamental and unnameable yearning after tactual terms. It is a mistake to say that the goal of science is the "continuum"—the paradoxical and inconceivable continuum. The continuum is really not a conception at all, it is merely a name applied to that feeling of vain and endless effort, that contradiction which we feel when we try to express or describe the conception of contact in visual terms. It is merely the term applied to the contradiction which arises from trying to exhaustively describe the original notion of contact by means of modifications of the notion of visible expanse. The various hypotheses of atoms, fluids, etc., are not true expressions of the notion of contact; they are really visual constructions of the imagination and are in their very nature incapable of defining it. They serve very well as arbitrary *signs* of this notion but when they are looked upon as anything more they are bound to lead to contradictions. But the important point to be observed is that the goal of the existence of such hypotheses, the one fundamental purpose for which they are constructed is to make contact possible. Thus the underlying presupposition of science is not the "continuum," but "tangibility." A tangible world is the kind of world it is striving to express. All things can be made clear, can be scientifically explained if they can be reduced to the type of the tangible.

In the sphere of philosophy the influence of the touch-world is not so apparent, and its importance is much less frequently asserted. It seems to be swamped, as was indicated before, by the character of the philosophic method. The tangible world cannot flourish on introspective and reflective soil. The introspective type of reflection which to such a great extent characterizes the current methods of philosophy, seems for the most part to be the visual type of knowledge, and stands just as incapable of describing the phenomena of touch as that of sound or any other sense. This visual type of knowledge recognizes the existence of the tangible world in the sense that it believes that there is a real world to express. But when it formulates a visual expression of this world, it begins to see that its visual lines have fallen in unpleasant places and present nothing more than a mass of abstractions and contradictions, such as the "continuum," "abstract substance," and all the other bug-bears of philosophy. This mass of absurdities it surely must discard; and mark just here how scepticism follows. Instead of calling in question its method of expression and seeing its inadequacy, it regards this so called "mass of absurdities" as a true expression of the real world, and consequently resorts to the conclusion that there is no real world at all—the real world having become identified with this chaotic expression is rejected with it. This method of philosophy is the foundation of the Berkeleyan type of idealism and the scepticism of Hume. The material world to which Berkeley meant to deny existence was not the world of touch but the chaotic offspring of the visual expression of that world, and in this he took a very important step towards clearing away the "philosophic dust;" but immediately afterwards he took a seriously false step in attributing the fault in this inadequate expression to the side of the touch-world rather than to the method of expression. In consequence of this he led the way to the denial that there was any real world to express, and this false step is carried out and fully developed with all its implications in the sceptical philosophy of Hume.

The thoroughgoing criticism of Hume marks the period for the beginning of a new system of philosophy. Hume boldly encountered the great paradox involved in the attempt to express the real world by the reflective method, accepted it as unavoidable, and denied the possibility of metaphysics. Now, if there is to be a new positive philosophy, this paradox must be solved, and this is possible, obviously, only on condition of a change in the philosophic method. In making this change there are two alternatives: either the reflective method must be retained and greatly modified and manipulated, or it must be abandoned altogether and the external world must be asserted from the side of its own special sense, which throughout this paper we have been trying to maintain is the sense of touch. Has philosophy ever attempted this? The tangible world we saw, forcibly asserts its influence and importance throughout the earliest stages of animal life, in the practical world of common sense, and in the domain of science. Now the question is, has it done so also in philosophy, or has it in this sphere been altogether neglected? Is there any evidence that there is any one system of philosophy whose characteristic method of procedure, whose characteristic type of thought we can identify with the type of touch?

We shall try to adduce evidence to show that what may be called the psychological foundation of the Scottish school of philosophy, Natural Realism, is the sense of touch; that the particular type of thought, or thought-temperament which is the underlying possibility of such a doctrine is the "touch type." Or to be more particular, what we shall try to prove is that the real external world which this school of philosophy so bravely defends, and tries so hard to express, is not a world known by some inexplicable divine intuitive act of consciousness as they thought, but the simple and hitherto unattended to, phenomena of the *special* sense of touch; and that the characteristic "immediate" type of knowledge by which they conceived this world to be known, can be identified with those processes which are peculiar to tactual perception.

The essential point to be noted in the doctrine of Natural Realism is that it is a reaction against the Lockian "theory of ideas." According to this theory of ideas all knowledge is mediate, we can only know things through their ideas. Now the school of Realism, noticing the sceptical outcome of this doctrine, reviews it, and finds that, though there is a great deal of truth in it, yet it is only a partial view. Realism says that "mediate" knowledge is not all; there is immediate knowledge; there is a certain kind of knowledge in which there is no *tertium quid*. Or again, Realism may be said to be a forcible return to perception. The Berkeleyan idealism reduced perception to the type of conception. Realism brings perception back to its original type and emphasizes it. The watch-word of the whole system is "immediate perception." And now that we have the doctrine as it were in a nut-shell, all we have to do is to find out what is really meant by "immediate perception"—what is the type of knowledge it expresses. In order to do this let us first see what sort of criticism the Scottish philosophers passed on the reflective method of idealism, and what method they proposed to put in its place.

The criticism they passed on the method of reflection was essentially psychological. They looked into the psychological basis of the method. And what do they find? That the whole system is built up on an analogy of visual processes. They analyze the language of philosophy and they find that it is made up almost wholly of visual terms.

Dugald Stuart says:

> "Another observation too, which was formerly hinted at, is confirmed by the same historical review; that in the order of inquiry, the phenomena of vision had first engaged the attention of philosophers, and had suggested to them the greater part of their language, with respect to perception in general; and that in consequence of this circumstance, the common modes of expression on the subject, unphilosophical and fanciful at best, even when applied to the sense of seeing, are in the case of all the other senses obviously unintelligible and self-contradictory."

Dr. Thomas Reid gives the same criticism of the so called idealistic method:

> "Of all analogies between the operations of body and those of the mind, there is none so strong and so obvious to all mankind as that which there is between painting or other plastic arts, and the power of conceiving objects in the mind. Hence, in all languages the words by which this power of the mind and its various modifications are expressed, are analogical and borrowed from those arts. We consider this power of the mind as a plastic power, by which we form to ourselves images of the objects of thought."
>
> "In vain should we attempt to avoid this analogical language, for we have no other language upon the subject; yet it is dangerous and apt to

mislead. All analogical and figurative words have a double meaning, and if we are not very much upon our guard, we slide insensibly from the borrowed and figurative meaning into the primative. We are prone to carry the parallel between the things compared farther than it will hold, and thus very naturally to fall into error."

The idealistic method of philosophy then, both Reid and Stuart recognize to be essentially of the visual type. All the current philosophical language is saturated with visual terms and becomes perfectly unintelligible when employed to express the phenomena of the other senses. Natural Realism has a great truth to express but it can find no language that will express it—the visual language of philosophy will grossly misrepresent it. This is the general criticism. But there is one central point in which this visual method shows its inadequacy to express the truth of Realism, and in this we can make the first step towards the psychological interpretation of what is meant by immediate perception. The place where idealism and realism part company once for all is in the distinction between the primary and secondary qualities of matter. Idealism makes no absolute distinction between them, and allows both alike to be expressed by its "ideas" or "visual images." The point upon which Realism insists is that there is something in the nature of the primary qualities that absolutely refuses to be expressed by the same method that expresses the nature of the secondary ones. This peculiarity is "the direct" and "distinct notion" which we get "of what they are in themselves." Dr. Reid expresses the distinction thus:

"Is there anything common to the primary which belongs not to the secondary? And what is it?

"I answer, that there appears to me to be a real foundation for the distinction; and it is this—that our senses give us a direct and a distinct notion of the primary qualities and inform us *what they are in themselves.* But of the secondary qualities our senses give only a relative and obscure notion. They inform us only, that they are qualities that effect us in a certain manner—that is, produce in us a certain sensation; but as to what they are in themselves, our senses leave us in the dark."—Reid's Collected Writings, edited by Hamilton. (seventh edition) Vol. I, p. 313.

This "direct notion of what things are in themselves" is what Reid means by immediate perception, as all who are acquainted with his philosophy will know; and from the above passage we learn that it is the peculiar type of knowledge by which we know the primary qualities as distinct from the secondary. This kind of knowledge, he maintains, cannot be reduced to the mediate type; it is a type which must be expressed after its own peculiar fashion. It can be seen from the following quotation from Reid that what is really meant here is tactual perception. Speaking of the difference between visible and tangible magnitude he says:

> "Such differences in their properties led Bishop Berkeley to think that visible and tangible magnitude and figure are things totally different and dissimilar, and cannot belong to the same object.
>
> "And upon this dissimilitude is grounded one of the strongest arguments by which his system is supported. For it may be said, if there be external objects which have a real extension and figure, it must be either tangible extension and figure, or visible, or both. The first appears absurd; nor was it ever maintained by any man that the same object has two kinds of extension and figure totally dissimilar. There is then only one of the two really in the object, and the other must be ideal. But no reason can be assigned why the perceptions of one sense should be real, while those of another are only ideal; and he who is persuaded that the objects of sight are ideas only, has equal reason to believe so of the objects of touch.
>
> "This argument, however, loses all its force, if it be true, as was formerly hinted, that visible figure and extension are only a partial conception, and the tangible figure and extension a more complete conception of that figure and extension which is really in the object."—*Essays on the Intellectual Powers of Man*, Collected Writings, I, 325.

In this passage the psychological interpretation of Reid's conflict with Berkeley is made very clear. Both agree that the visual and the tangible worlds are incommensurable as such, yet both want to give the world a homogeneous expression. In doing this they part company; Berkeley takes the visual world and makes the tangible conform to its type; Reid prefers the tangible and makes all conform to its type.

But we can make a more special analysis of what Natural Realism means by the intuitive conception of external reality. Dr. Reid distinguishes carefully between what he calls his "conception" of hardness and the "sensation" which accompanies the touching of a body.

> "Let a man press his hand against a hard body, and let him attend to the sensation he feels, excluding from his thought everything external, even the body that is the cause of his feeling. This abstraction indeed is difficult, and seems to have been little, if at all, practised. But it is not impossible, and it is evidently the only way to understand the nature of the sensation. A due attention to this sensation will satisfy him that it is no more like hardness in a body than the sensation of sound is like vibration in the sounding body.
>
> "I know of no ideas but my conceptions; and my ideas of hardness in a body is the conception of such a cohesion of its parts as requires great force to displace them. I have both the conception and belief of this quality in the body, at the same time that I have the sensation of pain by pressing my hand against it. The sensation and perception are closely conjoined by my constitution, but I am sure they have no similitude; I know no reason why one should be called the idea of the other, which does not lead us to call every natural effect the idea of its cause."—Reid's Collected Writings, edited by Hamilton, Vol. I, p. 317.

He presses his hand against a hard body, he feels certain sensations in his hand, temperature feelings, muscular feelings, feelings of fatigue, feelings of one part pressing against another, all of which he recognizes as some affection of hi hand and which he is pleased to call by the name of "sensation." But none of these gives him the "conception" of hardness; what he means by hardness is something very different and foreign to all of them. Yet accompanying these "sensations" he gets this "conception" of hardness; he gets it at the same time that he gets the subjective feeling in his hand; he knows not how, he simply gets it. Now does not all this look very much as if that which Reid called "conception" of hardness was just the *special* sensation of touch? He did not know that there was such a thing as a special sense of touch distinct from those other feelings which appear as affections of the skin, and what is more likely than that he should christen the feeling which he got from it by such a name as "intuitive conception?" But all this will be made clearer and more conclusive by the following passage:

> "There is, no doubt, a sensation by which we perceive a body to be hard or soft. This sensation of hardness may easily be had, by pressing one's hand against the table, and attending to the feeling that ensues, setting aside, as much as possible, all thought of the table and its qualities, or of any external thing. . . . .
>
> "There are, indeed, some cases, wherein it is no difficult matter to attend to the sensation occasioned by the hardness of a body; for instance, when it is so violent as to occasion considerable pain: then nature calls upon us to attend to it, and then we acknowledge that it is a mere sensation, and can only be in a sentient being. If a man runs his head with violence against a pillar, I appeal to him whether the pain he feels resembles the hardness of the stone, or if he can conceive anything like what he feels to be in an inanimate piece of matter.
>
> "The attention of the mind is here entirely turned towards the painful feeling; and, to speak in the common language of mankind, he feels nothing in the stone, but feels a violent pain in the head. It is quite otherwise when he leans his head gently against the pillar; for then he will tell you that he feels nothing in his head, but feels hardness in the stone. Hath he not a sensation in this case as well as in the other? Undoubtedly he hath; but it is a sensation which nature intended only as a sign of something in the stone; and, accordingly, he instantly fixes his attention upon the thing signified; and cannot without great difficulty, attend so much to the sensation as to be persuaded that there is any such thing distinct from the hardness it signifies.
>
> "But, however difficult it may be to attend to this fugitive sensation, to stop its rapid progress, and to disjoin it from the external quality of hardness, in whose shadow it is apt immediately to hide itself; this is what a philosopher by pains and practice must attain, otherwise it will be impossible for him to reason justly on this subject, of even to understand what is here advanced. For the last appeal, in subjects of this nature, must be to what a man feels and perceives in his own mind.
>
> "It is indeed strange that a sensation which we have every time we feel a body hard, and which, consequently, we can command as often and continue as long as we please, a sensation as distinct and as determinate as any other, should yet be so much unknown as never to have been made an object of thought and reflection, nor to have been honored with a name in any language; that philosophers, as well as the vulgar, should have entirely overlooked it, or confounded it with that quality of bodies which we call *hardness*, to which it hath not the least similitude. . . . .

"The firm cohesion of the parts of a body, is no more like that sensation by which I perceive it to be hard, than the vibration of a sonorous body is like the sound I hear: nor can I possibly perceive, by my reason, any connection between the one and the other. No man can give a reason, why the vibration of a body might not have given the sensation of smelling, and the effluvia of bodies affected our hearing, if it had so pleased our Maker. In like manner, no man can give a reason why the sensations of smell, or taste, or sound, might not have indicated hardness, as well as that sensation which, by our constitution, does indicate it. Indeed, no man can conceive any sensation to resemble any known quality of bodies. Nor can any man show, by any good argument, that all our sensations might not have been as they are, though no body, nor quality of body, had ever existed.

"Here, then, is a phenomenon of human nature, which comes to be resolved. Hardness of bodies is a thing that we conceive as distinctly, and believe as firmly, as anything in nature. We have no way of coming at this conception and belief, but by means of a certain sensation of touch, to which hardness hath not the least similitude; nor can we, by any rules of reasoning, infer the one from the other. The question is: How we come by this conception and belief?

"First, as to the conception: Shall we call it an idea of sensation, or of reflection? The last will not be affirmed; and as little can the first, unless we will call that an idea of sensation which hath no resemblance to any sensation. So that the origin of this idea of hardness, one of the most common and most distinct we have, is not to be found in all our systems of the mind: not even in those which have so copiously endeavoured to deduce all our notions from sensation and reflection.

"But, secondly, supposing we have got the conception of hardness, how came we by the belief of it? Is it self-evident from comparing the ideas, that such a sensation could not be felt, unless such a quality of bodies existed? No. Can it be proved by probable or certain arguments? No; it cannot. Have we got this belief, then, by tradition, by education, or by experience? No; it is not got in any of these ways. Shall we then throw off this belief as having no foundation in reason? Alas! it is not in our power; it triumphs over reason, and laughs at all the arguments of a philosopher. Even the author of the "Treatise of Human Nature," though he saw no reason for this belief, but many against it, could hardly conquer it in his speculative and solitary moments; at other times, he fairly yielded to it, and confesses that he found himself under a necessity to do so.

"What shall we say then of this conception, and this belief, which are so unaccountable and untractable? I see nothing left, but to conclude, that, by an original principle of our constitution, a certain sensation of touch both suggests to the mind the conception of hardness, and creates the belief of it." . . . .

"What hath been said of hardness, is so easily applicable, not only to its opposite, softness, but likewise to roughness and smoothness, to figure and motion, that we may be excused from making the application, which would only be a repetition of what hath been said. All these, by means of certain corresponding sensations of touch, are presented to the mind as real external qualities; the conception and the belief of them are invariably connected with the corresponding sensations, by an original principle of human nature."—*Inquiry into the Human Mind*, Collected Writings, Vol. I, p. 120.

In this passage there are noticeable two special points in which there is a most striking resemblance between Reid's "intuition of the primary qualities" and the special sense of contact. 1. In order to get the sensation of contact prope there must be a certain amount of pressure on the skin. If the pressure is very light we get the "tickle" sensation. On increasing the pressure within certain limits we get the sensation of contact proper—the feeling which is of the nature of the "hard kernel." On increasing the pressure still further we get the more subjective type of feelings, muscle-sense, pressure of the muscles against one another, tendon sensations and perhaps "innervation feelings," pain, etc. Now Reid, in this passage, is very careful to make plain that his "intuition" only accompanies that degree of pressure which is within the limits of the sense of contact. Can we desire any more conclusive circumstantial evidence that this indefinable "conception" or "intuition" is just the specific sense of touch? 2. The characteristic which most clearly distinguishes the sense of touch from all other senses, is that it is the final and ultimate appeal to reality. The reality which we get by all other senses has the characteristic of being *inferential*—we always can reason as to the real existence of what they inform us about—we can doubt its reality and often have reason to—but the reality of touch is ultimate; we can have no proof of it; it is its own proof, its reality is given immediately. The interest of touch is always practical, and never speculative. There is no separation of the sensation from the belief. How all this came about according to the principles of natural selection, we saw before. Now this is just the character of Reid's "intuition" of the real world. Reason or reflective thought may deny it, may ignore it, in its philosophical seclusion from active life, but in real life, in practical life, it laughs at reason. This "belief" type of knowledge, which was before shown to be the special character of the "touch" type, is the characteristic which distinguishes Natural Realism as a distinct system of philosophy; this can be seen from many such passages as the following:

> "We know what rests on reason, but believe what rests on authority. But reason itself must at last rest on authority, for the original data of reason do not rest on reason, but are necessarily accepted by reason on the authority of what is beyond itself. These data are therefore, in rigid propriety, Beliefs or Trusts. Thus it is that in the last resort we must perforce philosophically admit that belief is the primary condition of reason, and not reason the ultinate ground of belief." "The ultimate facts of consciousness are given less in the form of cognitions than of beliefs. Consciousness in its last analysis—in other words, our primary experience—is a faith. We do not in propriety *know* that what we are compelled to receive as not-self is not a perception of self; we can only on reflection *believe* each to be the case *in reliance on the original necessity of so believers imposed on us by nature.*"—HAMILTON, *Discussions*, p. 86.

Sir William Hamilton agrees with the fundamental principle of the doctrine as laid down by Reid and Stuart. He retains the doctrine of immediate perception but with some modification. The change which he makes, stated in a word, consists in narrowing down the amount of non-ego or external reality perceived, and the particular way in which he does this makes the evidence all the stronger that the "immediate perception" is in its ultimate analysis, the sense of contact. The tendency of Reid in his uncritical enthusiasm over his great truth, was to regard the immediate type of knowledge as extending over a very large area of thought, but Hamilton is more critical and makes an effort to find out its original meaning and to what particular sphere it belongs. To what extent do we have this intuitive perception of external reality, he asks, and what are the sole conditions on which it is possible? He discovers, in answer to this, that the object of perception, in so far as it is a quality of the extra-bodily world, is that which is in *contact* with the organ of sense. "An external object is only perceived inasmuch as it is in relation to our sense, and it is only in relation to our sense inasmuch as it is present to it." The only way any *real* external thing can affect us, is by actual contact. The only terms in which *reality* can express itself as such, are terms of contact. A few quotations will make this clear.

> "We perceive through no sense aught external, but what is in immediate relation and in immediate *contact* with its organ; and that is true which Democritus of old asserted, that all our senses are only modifications of touch. Through the eye we perceive nothing but the rays of light in relation to, and in contact with the retina; what we add to this perception must not be taken into account."—*Metaphysics*, Lecture XXV.

> "To say that we perceive the sun and moon is a false or elliptical expression. We perceive nothing but certain modifications of light in immediate relation to our organ of vision. It is not by perception, but by a process of reasoning, that we connect the objects of sense with existence beyond the sphere of immediate knowledge. It is enough that perception affords us the knowledge of the non-ego *at the point of sense*. To arrogate to it the power of immediately informing us of external things which are only the causes of the object we immediately perceive, is either positively erroneous or a confusion of language."—*Metaphysics*, Lecture, XXVII.

Is this not a strong point then, in favor of the position we are trying to support, that the most acute representative of the doctrine of Natural Realism, should find the only possibility of a direct knowledge of external reality, in the one sense of contact?

Very closely allied to the sense of contact is the muscle sense. We always find the most highly developed tactual appendages also the most mobile, and it is quite true that locomotor sensations play a great part as concomitants to the sense of touch in making up our knowledge of the external world. But touch nevertheless gives us the *essential* feature of our world; it supplies the content, as was indicted before by the experiment of Landry. The muscle sense may help us to say that there is an external world, but it is the sense of contact that says what world it is. Considering this close alliance and co-partnership between the two senses, we might expect a tendency on the part of Natural Realism to explain the notion of external reality on the basis of the sense of resistance to effort. And this is just what we find. Hamilton in his later writings drifts towards this idea. Yet, in his case, it is quite evident from his doctrine as a whole, that what he really means is resistance *plus contact*. Just in so far as he would mean simple muscular resistance without the sense of contact he would not be a Natural Realist, as is well seen in the following criticism by Professor Veitch, a typical Natural Realist, on this very tendency of Hamilton's later thoughts:

> "It seems doubtful whether the apprehension of resistance or of a resisting something as extra-organic in the locomotive effort is fitted or sufficient to give the intuition of extension or an extended thing. The intuition of resistance might be quite well satisfied by a force—a degree or intension of force—in correlation with the organism. Electricity would be sufficient to impede the locomotive effort; yet we should hardly regard this as adequate to give us the intuition of an extended object, though it might be apprehended as external. These considerations tend to show that the locomotive power has received somewhat exaggerated importance as a factor in our apprehension of extra-organic objects. The three sources of knowledge—Contact, Pressure, and Locomotion—seem to me to be required to go together, and yield a conjoint result, ere we can form the complex notion of body,—as external, extended, and resisting."—VEITCH, *Hamilton*, Blackwood's Philosophical Classics, 141, Glasgow 1888.

The external world for which the Scottish philosophers are contending, then, is not a world that can be inferred from muscular resistance; it may be known in connection with this resistance but is not derivable from it. This point is brought out even more emphatically in the criticism of Natural Realism proper on the doctrine of Inferential Realism as given by Dr. Thomas Brown. The pith of Brown's doctrine can be seen from the following quotation:

> "To what, then, are we to ascribe the belief of external reality which now accompanies our sensations of touch? It appears to me to depend on the feeling of resistance, which, breaking in without any known cause of difference on an accustomed series of feelings, and combining with the notion of extension, and consequently of divisibility, previously acquired, furnishes the elements of that compound notion which we term the notion of matter. Extension and resistance—to combine these simple notions *in something which is not ourselves*, and to have the notion of matter, are precisely the same thing."—L. XXIV., p. 150.

The following is the criticism of Natural Realism as given by Professor Veitch:

> "This is a singular and glaring specimen of *petitio principii*. Whence our belief in external or non-mental existence? Extension and resistance are "feelings," "notions," subjective states merely. These combined can but constitute a more complex mental state. This is not an external reality,—it is not the matter Brown is in search of. But he quietly adds, "to combine these simple notions in something which is not ourselves, and to have the notion of matter, are precisely the same thing." But when and how do we get this "something which is not ourselves," this "something" which is over and above our sensations? This is not explained; it is assumed. . . . . But Brown's inference of a cause of resistance in something that is not self, is wholly unwarranted on the premises and by the process here given. (1) It is supposed to be reached on the principle, assumed to be intuitive, of similar antecedents having similar consequents. When antecedents are similar, consequents are similar; true, but for all this there may be events which have no antecedents at all in the case, it will be in virtue, first of all, of the principle that every event or change in our experience has a cause—a cause of some sort. This principle or necessity is not involved in the principle, that where antecedents are similar, consequents are similar; on the contrary, this latter principle is founded on the other as one at least of its essential elements. (2) But if we carry out our inference on the principle of difference of antecedent from difference of consequent, the antecedent inferred will still necessarily be one within our experience, not a something wholly unknown to us, of which we cannot predict either affirmatively or negatively. I have the feeling of resistance; I know nothing more; I have no right to speak of "some object opposed to me." This is to introduce an object which is not a sensation. But why speak here of an antecedent at all? There is even no antecedent in time here. The feeling of resistance is not, ex hypothesi, preceded in my states of consciousness by anything I know, or any state of consciousness. It arises suddenly, unexpectedly, from nothing known to me that has gone before. I have no known antecedent to fall back upon; and as my whole knowledge or consciousness in the matter is limited to antecedents which are states of my own mind, I ought naturally to seek the antecedent among these, not in the wholly new notion of something opposed to me,—some object which is not myself,—an object which transcends alike my experience and my knowledge. If I do reach this notion, I certainly do not get it by the principle of the similarity of sequence between antecedents and consequents. And just as little can I reach it by the principle of casuality. This principle might tell me there is *a* cause of the feeling of resistance; it could never tell me *what* that cause is, or give to me the new notion of a particular cause. . . . . Any form of cause—spiritual or material alike—satisfies the idea of cause. How then can I thus account for this belief in corporeal substance distinct from myself. Obviously, the whole process is a mere fallacy. And if we have this belief which Brown assumes, it never arose in the way he supposes it did. We have no alternative but to retrace our steps, and to admit with Hamilton that we have illegitimately rendered the immediate perception or intuition of the external object from the irresistable belief in it; that, in fact, we believe in an outward world in space because we know an outward world there, and believe that we know it."—VEITCH, *Hamilton*, Blackwoods Philosophical Classics, 168, Glasgow 1888.

Let us stop here with the consideration of the more particular points of evidence, and let us look at the whole matter from a general standpoint. From the main characteristics or general symptoms of the doctrine of Natural Realism, what is it that we find? Here is a peculiar doctrine of the perception of the external world—the statement of a peculiar type of perception. The essential feature of the world known by this kind of perception, is that it is not deducible or derivable from anything that we know; it is not derivable from the feeling of muscular movement, resistance to muscular movement, nor from any possible combination of any of the sensations which accompany it; it is known *directly* and in itself, its own nature is the only thing that can define it. Now of what are these the symptoms? Psychology only knows of one thing in psychic life which presents these same marks, and that is what it calls the "special sense." The special phenomena of the sense of sight though scientifically *explained* by the notion of waves of ether, can never be deduced from such a notion, or known in any other way except by means of "seeing." Does not this peculiar kind of perception of Natural Realism then, look very much like perception by some special sense? Supposing that we take for granted that it is so, is there any further evidence on the matter? Is there any evidence which goes towards defining what that special sense is? From the passages already quoted it is evident that the sphere to which this realistic perception originally belongs is the knowledge of the primary qualities of matter; that it originally and properly accompanies certain sensations of the skin; that the proper conditions on which it occurs, are identical with the conditions which by experiment have been found necessary to the specific sensation of contact; and lastly, that the essential feature in the content of this perception is identical with the content of the special sense of touch. Is it not likely then, that what the advocates of Natural Realism really meant by the immediate perception of reality was, though indeed they were far from being aware of the fact, nothing more or less than tactual perception? If we did not know that there was such a thing as the special sense of sight, and if all we knew in connection with visual perception as sensation were the muscular sensations of the eye, it is quite probable that we would have a whole system of philosophy equal to that of Natural Realism and full of magic categories, all built up for the purpose of explaining the simple peculiarity of visual phenomena. And from the considerations which have just been enumerated it is reasonable to suppose that on just such principles has the central principle of the philosophy of Natural Realism been established for the purpose of expressing the peculiar, specific nature of the special phenomena of touch.

The results may be summarized briefly as follows:— 1. There is a general distinction between reflective and practical thought, the characteristic of the latter being that it consists in a great complexity of reactions to the belief in an external reality. 2. The psychological foundation of this practical sphere of thought can be found in the sense of touch. 3. The conception of contact has great influence on scientific hypotheses. 4. Owing to the current method and language of philosophy the influence and importance of touch in this sphere is not so apparent and has for the most part been overlooked, but there is one system in the history of philosophy which has endeavored to assert its claims, viz: Natural Realism. What the Scottish philosophers Reid, Stuart, and Hamilton, were striving to express in their doctrine of "immediate perception" of external reality was really tactual perception.

Valuable suggestions can be drawn both for psychology and for philosophy. From the psychological side we see the pressing need of a thorough investigation into the dermal and locomotor senses. These undoubtedly, form the greater part of the basis of practical life, and yet, uninvestigated and without language in which to assert their importance, they are almost wholly overlooked in the current interpretations. The language of touch is at best very vague and general. Moreover it is not indigenous, but formed analogically from the terms of other senses. What it needs is a language of its own, and the way to this is obviously by the methods of experimental psychology and a study of the evolution of the senses. The variety of senses included under what is generally termed the tactuomotor sense must be separated by experiment, the special nature and function of each determined, and the primitive history of their relations to life and the other senses thoroughly investigated.

From the side of philosophy we can in the first place agree with the Scottish philosophers that the philosophical systems current at their time, systems founded on the Lockian theory of ideas, were one-sided; that they were constructed almost wholly out of analogies taken from visual phenomena and consequently unintelligible and self-contradictory when employed in the expression of truths received by means of other senses. Natural Realism had a great truth to impress upon the world, but owing to the corruption of philosophical method and language it failed to get a hearing. It has been eclipsed in the history of philosophy by other apparently more attractive systems, not owing to the fact that they had the balance of truth on their side but that they had the balance of language.

But the above results, I think, suggest a further and more important question as to the method of philosophy as a whole. If the fundamental category of the Lockian school of philosophy, the "idea," is really, as Reid and Stuart suggested, the "visual image," and if the central category of the Scottish school, "intuition" or "immediate perception" of reality is tactual perception, are we not led from this at least to hope for the possibility of finding a similar psychological statement for the chief categories underlying all systems of philosophy? Already it is generally admitted among philosophical critics that very large portions of every system of philosophy have a psychological foundation in the character of the age, nation and individual, and is it not equally probable that there is a like psychological basis underlying the very heart of each system, even unto the most fundamental and apparently ultimate categories? May it not be that all the magic categories of philosophy which profess to be ultimate expressions of the absolute, are only poetic attempts to express special feelings of sense which for want of attention and proper analysis are not recognized as such? The categories which have been handed down through the history of philosophy, taken as they must have been, from a comparatively chaotic mass of sensations, the elements of which were unseparated and uncriticized, must at best be very vague in their meaning and extremely inadequate to a scientific expression of the principles of life and thought. If a necessary element in the aim of philosophy is to keep itself in constant touch with the poetic æther, then its present system of categories may serve its purpose best, but if it means to bear a truly scientific attitude towards the world, it must forthwith surrender them up for psychological criticism, and that done, have itself restated in new and more scientific terms, rebuilt on a fresh system of more tangible categories all gotten from a thorough-going scientific analysis of instinct and sensation.

In conclusion I desire to express my gratitude to President Hall for kind criticism of my work and inspiriting suggestions during the preparation of this paper. I am also indebted to Dr. Scripture for reading over the paper and suggesting several corrections.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

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OBERSTEINER, *Die neueren Anschauungen über den Aufbau des Nervensystems*, Naturwissenchaftliche Rundschau, 1892, VIII, Nos. 1 and 2.

In less than half a dozen pages the author gives a very clear and judicial statement of the newer observations and theories which are the present guiding lines for research in the anatomy of the nervous system. Further comment is unnecessary except a word on a new term which appears in the article. For the most part anatomists take the view that the nerve cell and the nerve-fibre form a physiological unit and anatomically it is quite impossible to determine where one stops and the other begins. For the nerve cell and all its prolongations Waldeyer has suggested the term, Neuron. This fills so long felt a want and fills it so well, that there can be little question of its acceptance and hence the word of explanation.

LANGLEY AND SHERRINGTON, *On pilo-motor nerves*, Journal of Physiology, 1891, XII, 278.

The authors designate as pilo-motor those nerves which control the erection of the hairs or to use a single word, cause horripilation.

The experiments were made on a monkey, a young female Macacus rhoesus, and on cats. In all cases these nerves issue from the spinal cord by way of the ventral nerve roots and pass into the sympathetic ganglia; from there they are distributed to the skin.

The special arrangements are as follows: in the monkey the pilo-motor nerve-fibres for the head and face arise mainly from the third and fourth and less numerously from the second and fifth thoracic nerves. They pass cephalad in the cervical sympathetic and are connected with nerve cells in the superior cervical ganglion.

On stimulating the sympathetic nerve horripilation (in head and face) occurs chiefly on the homonymous side, but at the same time crosses the middle line to some extent.

On section of the sympathetic nerve the hairs lie abnormally flat in the effected region and remain so for many weeks.

In such a monkey anger and fear cause horripilation on the sound side only. The pilo-motor nerve-fibres issue in the roots of the twelfth thoracic, first, second, and third lumbar nerves, pass into the lumbo-sacral sympathetic chain and descend in it.

In the cat the pilo-motor nerve fibres are found in each nerve from the fourth thoracic to the third lumbar inclusive, sometimes also in the third thoracic. The fibres from the third or fourth to the seventh thoracic inclusive, run cephalad in the cervical sympathetic, join cells in the superior cervical ganglion, and innervate the skin on the head and on the back of the neck. These fibres are either not present or not functional in all the cats examined.

The pilo-motor fibres from the seventh thoracic to the third lumbar nerves, supply a strip of skin about twelve cm. wide, extending down the middle of the back from the upper part of the thoracic region to a point some six cm. out on the tail.

The plan of innervation in this region is very interesting. It can be shown that stimulation of any spinal nerve root in this group causes horripilation along a strip of skin some ten cm. in length. Taking any two successive nerves the more caudal one innervates a strip of skin the beginning and end of which are about two cm. caudal of the strip innervated by the more cephalic nerve.

1. BECK, *Die Bestimmung der Localisation der Gehirn-und Rückenmarksfunctionen vermittelst der elektrischen Erscheinungen*, Centralbl. f. Physiol. 1890 IV 473.

2. FLEISCHL v. MARXOW, *Mittheilung, betreffend die Physiologie der Hirnrinde*, Ibid. 1890 IV 537.

3. BECK, *Die Ströme der Nervencentren*, Ibid. 1890 IV 572.

4. GOTCH UND HORSLEY, *Ueber den Gebrauch der Elektricität für die Localisirung der Erregungserscheinungen im Centralnervensystem*, Ibid. 1891 IV 649.

5. DANILEWSKY, *Zur Frage über die elektromotorischen Vorgänge im Gehirn als Ausdruck seines Thätigkeitszustandes*, Ibid. 1891 V 1.

The above mentioned papers are experimental, polemical and historical. They have grown out of the question, how far the activity of the central nervous system is accompanied by demonstrable electrical changes, and to what degree these changes can be used for the study of localization of function in it. Gotch and Horsley stimulated the cerebral cortex and noted the electrical changes in certain tracts of the spinal cord.

The others have for the most part applied a peripheral stimulus and noted the electromotive changes in the brain, mainly in the cortex. From the results of all, it would appear that the cortex is usually active to such an extent that there are continuous and irregular electrical changes, which can not be accounted for by distinct peripheral stimuli. Peripheral stimuli produce more or less marked changes in the resting current taken from the cortex and there seems to be some relation between the disturbance in the several sensory cortical centres and stimulation applied to their appropriate sense organs, but it is far from precise or satisfactory. On the power of anæsthetics (chloroform and ether) to prevent these electromotive changes, the authors are not in accord, Beck claiming that the spontaneous activity of the cortex continues under choloform, while v. Marxow claims that the cortex is paralyzed by anæsthetics.

All those who have employed the "negative variation" as an instrument wherewith to attack physiological problems are aware that it is a hard one to handle, and whether it can be used to add to knowledge of the functions of the cerebral cortex remains yet to be shown.

STEWART, *Notes on some applications in physiology of the "resistance" method of measuring temperature, with special reference to the question of heat production in mammalian nerves during excitation*, Journal of Physiology, 1891, XII 409.

The apparatus used registered changes in temperature by the swing of a galvanometer needle and in most experiments variations of 0.0005° C. could have been detected with certainty. Neither in frogs nor dogs and rabbits is there evidence of a variation of the above mentioned amount in stimulated nerves. Hence such temperature changes as accompany the excitation of living nerves in these animals must be extremely small, if they occur at all. Likewise, nerves in the process of dying fail to show a change of temperature.

As bearing on the question of "thermogenic" as distinguished from "motor" nerves, it appears that the temperature of a muscle poisoned with curara does not rise on stimulation of the nerve, indicating that not only do the nervous impulses causing contraction of the muscle, but also those causing rise of temperature, (and they may or may not be one and the same) fail to effect the muscle, after curara.

The Johns Hopkins Hospital Reports, 1891, II, No. 6. *Report in Neurology, I.*

1. BERKLEY.—A case of Chorea insaniens, with a contribution to the germ theory of chorea.
2. SIMON.—Acute angio-neurotic oedema.
3. HOCH.—Haematomyelia.
4. THOMAS.—A case of cerebro-spinal syphilis, with an unusual lesion in the spinal cord.

In the papers above cited, the clinical and pathological points of view, as contrasted with the anatomical, are most emphasized. It will therefore be sufficient to mention here a few facts of very general interest connected with them. The study of chorea (1) is based on two cases—one a dog. In the first case towards the end of life, the chorea was associated with mental confusion. The post-mortem appearances to which the most value is attached were in the meninges and vessels, and are interpreted as the result of the action of a pathogenic germ or its products. To the numerous small extravasation of red blood corpuscles found in the nerve substances, but little significence is attached. Towards the end of the paper the changes occuring in the liver and kidney in diphtheria, are compared with those in the meninges, brain and kidneys in chorea, with a view to emphasizing the similarities and thus furnishing indirect evidence for the germ theory of chorea.

The disease designated as acute angio-neurotic oedema (2) is characterized by rather circumscribed swellings, appearing suddenly and often periodically, usually multiple and affecting the eyelids, lips, hands, feet, genitals, and buttocks by preference. There is often profuse vomiting. Three cases are carefully described. Vaso-mator influences alone appear insufficient to explain all the results, but as the disturbance is credited to the sympathetic system, these vaso-motor influences must be considered as one factor at least.

In the discussion of Haematomyelia (3) it is pointed out that hemorrhage into the spinal cord, not produced by trauma, is very rare. In the two cases described, while trauma is by no means excluded, yet the paralysis did not appear in one case until six days, and in the other until three weeks after the accident. The particular muscles affected were carefully studied, and from the probable location of the lesion the spinal centres for these several muscles is inferred, the inferences being controlled by what is already established in the localization of arm centres in the cord.

Dr. Thomas's case (4) yields the following anatomical summary. "Syphilitic orchitis. Syphilitic endarteritis (gumnatous) of cerebral arteries. Gumma on left third nerve involving crus. Gummata on left fourth, right sixth, ninth and twelfth nerves, and in brain. Gumma on anterior roots of three cervical nerves. Meningitis of cord. Poliomyelitis of lumbar enlargement. Hyaline degeneration in the walls of the small arteries." In the faithful account, both clinical and anatomical, which is given there, are a number of interesting points. No symptoms were observed which corresponded with the marked changes found in the lumbar cord. The tumor on the right sixth nerve should have caused paralysis of the external rectus muscle of the right eye. The right eye had been tested shortly before death but no such paralysis was observed. On the peripheral side of the gumma this nerve did contain a number of well preserved nerve fibres, and this, too, in spite of the fact that the fibres could not be traced through the tumor. In the first place it is remarkable that the nerve should not have been destroyed, and in the second place that it should transmit despite the fact that the nerve fibre could not be traced through it.

WALDEYER, *Ueber einige neuere Forschungen im Gebiete der Anatomie des Centralnervensystems*, Deutsche med. Wochenschr. 1891 XVII 1213, 1244, 1267, 1287, 1331, 1352.

This review of recent work on the finer anatomy of the nervous system is from the hand of an acknowledged master. It is intended to show how far the improvements in the histological technique have, during the past few years, revolutionized the views on the architecture of the nervous system. The first paper starts with a historical review of the subject up to the year 1880, throwing into a scheme the ideas then current. Next follows a statement of Golgi's principle results. Undisputed is his observation that the nerve process (axis-cylinder process or prolongation) is branched, and that in certain cases it branches so much as to lose its identity within the gray matter about it. Disputed are his interpretation of the two sorts of cells as sensory and motor and his hypothesis that the branches from the nerve-process form a morphologically continuous net work throughout the nervous system and that the protoplasmic processes are purely nutritive in function. The points of difference in the views of the brothers Ramon y Cajal and Golgi are clearly stated. One very important point in the conception of the nerve cell is the value to be attached to the protoplasmic prolongations. There is much to be said in favor of the view that they possess functions not dissimilar in kind from those of the nerve process.

In order to form a picture of the arrangement of the elements in the spinal cord it is to be remembered that we have to deal within the cord with (1) commissure-cells (Commissurenzellen), (2) the column-cells (Strangzellen), (3) the nerve-root cells (Nervenwurzelzellen), (4) the cells of the dorsal cornua with the diffusely branching nerve-process. Outside of the cord lie the cells of the spinal ganglia. In general the relation of these elements appears to be the following: The fibres of the dorsal roots—for the most part taking origin from the ganglia of the dorsal root—enter the cord and there divide into an ascending and a descending ramus. From the rami arise at short intervals the so-called collateral branches which penetrate the gray substance and end in terminal brushes. The relation of these terminations to the cells is a close one but nevertheless not that of continuity. To follow the relations topographically, there are in the dorsal cornua the cells with the diffusely branching nerve process, the function of which is not evident. In the column of Clark, or Stilling's dorsal nucleus—as Waldeyer prefers to call it—the cells appear of the group designated as column-cells. Their nerve process passes to the lateral column and they may or may not divide into an ascending and a descending ramus. If undivided the fibre turns cephalad and, in any case, gives off collaterals along its course. In most parts of the gray matter cells of this class are to be found. The commissure-cells differ from the last only in the fact that the nerves coming from them cross the middle line by way of the ventral commissure before they turn longitudinally.

The root fibre cells form a final group. In most of them the nerve-process gives off but a small number of branches near its origin and in some animals none at all have yet been discovered. The nerve processes for the most part pass into the ventral nerve roots but in some instances a group of cells mainly in the lateral portion of the ventral cornua send their nerve-processes to the dorsal roots. Thus some fibres in the dorsal roots arise from cells within the cord, a fact for which there has long been good pathological evidence. In the third paper the relations of the nerve elements in the cortex of the cerebellum and cerebrum is discussed. Some account of the relations in the cerebellum has already been given (this JOURNAL, Vol. III, No. 4, Feb. 1891). The account of matter in the cerebrum is taken from Ramon. The large pyramids whose pyramidal prolongation with its protoplasmic branches may extend almost to the sub-pial layer has a nerve-process which may contribute to any of the principal fibre-systems of the hemispheres and the branches of which also form modulated fibres as shown by Flechsig. Above and below the large pyramids lie triangular or small pyramidal cells, the lower layer of which have the peculiarity that their nerve process is directed towards the cortex. In the cortex of the rabbit S. Ramon has found bipolar cells and also triangular ones with several nerve prolongations both of which are entirely new elements in this region. In the fourth article it is principally the comparative anatomy of the nerve elements which is considered, especial weight being laid on the relations existing in the invertebrates. Without sufficient grounds, as it seems to us, Waldeyer concludes in favor of direct anastomoses between cells in both vertebrates and invertebrates. In the crustacea which he examined Retzius considers almost all the ganglion cells as monopolar. The nerve process however has lateral branches, but no nerve fiber takes its origin from these branches. In the crabs the sensory and motor fibres appear to arise in the same manner and we have then both sorts of cells centrally located and in physiological connection by the lateral branches of the nerve-process.

These general views find support in the arrangements within both the olfactory and optic apparatus. Here there is evidence for two sets of fibres originating from separated groups of cells and running in opposite directions. In the case of the olfactory it would appear that some cells of the sensory nasal epithelium are cells of origin for nerve fibres passing from them to the bulb. One question of prime importance in connection with the supporting tissues of the nervous system is whether these are derived from the epiblast alone or from this layer and the mesoblast. This is one of the matters discussed in the fifth paper and although it must be still left open, the evidence appears to be strong that these tissues have a double origin. In the final paper the various points discussed are brought into a general view and illustrated by schemata. Some of the principle conclusions are the following. The axis-cylinder of all nerve fibres is the direct outgrowth of a nerve cell and in no case does it arise out of a net work of fibres. All nerve fibres end free in a terminal-brush and in no case is there formed an anastomosing net-work. The entire nervous system can be considered as built of a series of units. Each unit is the cell and its outgrowths. This unit Waldeyer designates as a neuron. As a rule the order of arrangement is such that physiological connection is established by the terminal brush of one neuron expanding in the neighborhood of the cell-body of the next. The arrangement in the glomeruli of the olfactory bulb suggests that in certain cases the terminal brushes of two nervous-processes may be directly approximated. In considering the value of the nerve cells, Waldeyer discusses the hypothesis of Nansen that the nerve cells are simply nutritive and do not form part of the pathway for the nervous impulses, coming to the conclusion that the evidence for Nansen's view is at present insufficient.

Two very general matters may be mentioned in conclusion. First, up to recently it has been generally held that the method of silver impregnation depended solely on a deposit of metal in lymph-spaces, to this Waldeyer adds a possible staining of certain elements which if it means anything means that the reaction takes place within the substance stained and not around it. Second, Kölliker in discussing this subject has laid great stress on the question how far the fibres brought out represent those which are modulated and how far those which are non-modulated. On this point Waldeyer has nothing to say.

OBERSTEINER, *Anleitung beim Studium der Baues der nervösen Centralorgane im gesunden und kranken Zustand*, 2. Aufl., Leipzig 1892.

The first edition of this admirable work was received with general rejoicings and was, at the time, reviewed in these columns. (AM. JOUR. OF PSYCHOLOGY, Vol. II, No. 2, Feb. 1889.) Since then (1890) it has undergone translation at the hands of Dr. Alex. Hill, of Downing College, Cambridge, England. The translation is good and the English edition differs from the original German in containing certain addenda, (always bracketed in the text), in which, for the most part, the translator presents some morphological views of his own. We do not propose to attempt here more than to point out some features of the second German edition as compared with the first. The fundamental character and arrangement are unchanged; as the author tells us in the preface, the text has been carefully worked over. The result is about one hundred pages more of reading matter and several new cuts.

A first-class book of this kind is in some sense a work of art and as such must have its sketchy portions. At the same time it is sure to be judged by what is best in it which, in this case, is the anatomical matter —in the stricter sense of the term. Where the evidence for views is physiological or developmental, the author's critical sense is less helpful to the reader. We have said that the book has grown and that in parts it is sketchy; it is to be devoutly hoped that it may remain so and stop growing. Even in this second edition there are introduced new things, presumably for the sake of completeness, which weaken its character as a critical essay. It seems the fate of many strong books to thus undergo in later editions a form of fatty degeneration where bulk is gained and tone is lost, and the perspective of the subject is damaged. Turning now to details, several matters call for notice.

The section on methods is fuller and more accurate than before. Take it all in all this chapter forms the best manual on the histological methods for the nervous system that we have. In discussing the method of degeneration it does not appear why Schwalbe's hypothetical nerve fibre with two nutritive centres should be introduced. It represents a purely formal difficulty. The development of the central nervous system is just touched upon and histogensis is hardly mentioned. In the chapter in morphology Fig. 20 is not without fault. The lateral plexus appears to be cut off from the rest of the velum and the stria cornea is represented on one side only.

In considering the fissuration of the hemispheres the author holds closely to Ecker. Eberstaller's contributions to the subject are recognized in the text and we should be glad to see his boundaries for the occipital lobe accepted in the figures. These figures (24-27) can certainly be improved. The central fissure should cut the mantel-edge and the relations of the interparietal sulcus and parieto-occipital fissure in Fig. 24 are quite misleading. The parieto-occipital fissure normally cuts the mantel-edge much in front of the point at which it is indicated. And furthermore the figures do not agree among themselves in representing this relation.

At birth the fissuration in the normal brain is usually almost complete, only the tertiary sulci being in part undeveloped and the statement that "the principal fissures are present," leaves the completeness quite unemphasized; for the erroneous idea that the fissuration of the hemispheres at birth was still far from finished, Ecker is mainly responsible.

In alluding to localization in the cortex Obersteiner falls back on Exner's view that they are foci without sharp limits. However true this may be for the lower mammalia, the recent work on man and the higher monkeys points to a sharper limitation in these higher forms, so far, at least, as the motor centres are concerned. The law given for the relation of the thickness of the cortex to the size of the gyri, according to which the larger gyri have the thicker cortex, certainly does not include the insula, for there the gyri, however considered, are of moderate size though the cortex is the thickest. Further, deep sulci are found in the occipital region where the cortex is thin, and gyri with broad tops, though bounded by shallow sulci, are found on the orbital surface where the cortex is equally thin. The large gyri in this connection must therefore have both broad tops and deep sulci bounding them in order to present the thicker cortex.

In discussing brain weight, if the figures are taken from Bischoff, as they appear to be, the average weight for the female brain should be 1220 grms. instead of 1230 grms. as printed. It should further be made clear that these mean weights are obtained from brains still enclosed in the leptomeninges.

Direct evidence given by Topinard, Bischoff, Boyd and others indicates that some brain growth takes place up to thirty-years in females, while in males it may continue nearly ten years longer. This fact is hardly suggested by the statement that towards the twentieth year the maximum brain weight is attained. In presenting Wagner's figures for the superficial extent of the cortex it should be stated that the measurements were made on brains shrunken by alcohol and that while the specimens of Wagner's series are comparable among themselves, his figures do not without correction form a basis for determining the extent of the cortex in the fresh brain.

It is agreeable to find the question of the difference in the weight of the two hemispheres of the brain properly neglected. There is no doubt that some brains have one hemisphere larger than the other. There is equally little doubt that the differences usually found depend on the difficulty of dividing the brain fairly and that these difficulties, arising from the distortion of the specimen and the constant error of the operator—who cannot possibly divide even a favorable object into two equal portions,—give rise to those inequalities which have so often been treated as important, but which in reality lie well within the limit of the errors of observation. The results of Obersteiner's own careful observations on the specific gravity of different portions of the brain form the last topic discussed in this chapter on the morphology. In taking up the histological elements of the nervous system the author states the fibrilla and hyaloplasma theories of the structure of the axis-cylinder without attempting to decide between them. While considering the axis-cylinder as continuous he decides, on the strength of Jacobi's results, against the discontinuity of the sheath of Schwann, as advocated by Boveri. In man he claims a considerable degeneration of fibres on the central side of lesions occuring in a peripheral nerve,—adding that this degeneration is less marked in animals. Further a normal degeneration of peripheral nerves in man, implying a degeneration of the cells connected with them, is asserted. Such an idea surely needs more evidence for its support than can at present be furnished. In discussing the nerve-cell he adheres to the older views. The absence of chromatin from the nucleus of the nerve cell is noted and the cell outgrowths discussed with special reference to the axis-cylinder prolongation as demonstrated by Golgi's method. The author seems to us unduly skeptical on this particular point, though his arguments against the simple nutritious functions of the protoplasmic prolongations have more force. Our principle indictments against this chapter is that he still speaks of nerve-cells and fibres as separate elements, thus failing to utilize the valuable conception of the cell and its fibres as forming both a morphological and physiological unit.

Beginning with the spinal cord we come to the most valuable portions of the book. Here Cajal's results are freely used. The view that the columns of Goll are pathways for the muscle sense is supported by the observation that these columns are poorly developed in the limbless forms. The segmental nature of the spinal cord is passed over on the ground that it is but faintly indicated in the higher mammals—not a very sufficient reason.

In discussing the spinal nerves their double origin—from both sides of the cord—is described and this idea is carried over to the cranial nerves where even the patheticus and abducens are forced into the schema. One cannot help feeling in the light of v. Guddins's results, that the weight of evidence is against such a view. The new figures (134-136) in the section on the medulla and interbrain form welcome illustrations of the latter region. If an argument were needed to show how much the histologist had yet to do on the nerve centres, no better one could be offered than the fact that the olfactory bulb and tract are here illustrated and considered from the examination of them in the dog. The contribution of His to the make up of the olfactory bulb is not mentioned and the double nature of the optic nerve is passed over. Farther on, the anatomical myth about the fibres of the Callosum joining identical points of the cortex appears. This is pure hypothesis and should not be presented as anything else.

Finally, the pictures illustrating the cortex (p. 445) are all out of drawing. The size of the cells and the relative thickness of the several layers are both calculated to give wrong impressions, which are only in part to be corrected by the figure on p. 451 illustrating the distinction of fibres in the cortex.

References to the more important literature have been introduced at the end of each section and in many cases the abbreviations used in connection with the figures have been arranged in alphabetical order in the explanation, thus facilitating reference. The foregoing remarks are intended simply as a running comment to the thanks due the author from those who have occasion to use his lucid and instructive book.

TURNER, *The convolutions of the brain; a study in comparative anatomy*, Jour. of Anat. and Physiol., 1890-1, XXV, 105, also *Verhandlungen des X. internationalen medicinischen Congresses, Berlin 1890*, II. Berlin 1891.

This paper is valuable for the simple and novel form in which it presents the comparative anatomy of the gyri. Lacking, as we do, a really adequate theory of the formation of the gyri from the physiological side, it is necessary to come back to the comparative anatomy for the significance of these foldings; from this latter standpoint our author reviews the field.

He makes departure from the very general fact that a cerebral hemisphere is separable into two natural divisions—a ventral portion, or Rhinencephalon, and a dorsal portion, or Pallium. These main divisions are separated by the rhinal or ects-rhinal fissure.

So far as the rhinencephalon is concerned Turner follows Broca in making it the basis for a further grouping. Instead of Broca's two groups of osmatic and anosmatic animals, Turner makes a threefold division into macrosmatic, represented by the Ungulata, Carnivora, etc., microsmatic represented by the Pinnipedia, whalebone-whales, apes and man, and finally the anosmatic, represented by the dolphins, toothed-whales, etc.

In ascending the mammalian series the pallium developes the more rapidly and thus more and more overgrows the rhinencephalon. As a result the rhinal fissure passes from a lateral position in the lower forms, to a ventral one, in the higher. The main subdivisions of the rhinencephalon are the bulb, peduncle and lobus hippocampi. Two roots of the peduncle are described; these bound the quadrilateral space. Of course with the variations in the size of both pallium and rhinencephalon the topographical relations of the latter may be various. The lobus hippocampi is in general larger in the microsmatic animals than in the anosmatic, but it is still present in the gross form in the latter. Supposing that in this last case it contains normal nerve elements, we are in the position of being forced to explain a special-sense centre which has no peripheral connections. There seem two ways out of such a position; either to find that the hippocampal lobes are histologically abnormal in the anosmatic forms or that this region has some other function in addition to a centre for smell. It is not improbable that both these notions would be involved in any complete explanation. In all animals the pallium is larger than the rhinencephalon, the difference being greatest in the higher forms. Species in the same order may have in some cases a convoluted pallium, in others a smooth one. The insectivora are apparently the group in which the surface of the hemispheres most perfectly preserves its smoothness throughout life in all the genera. In the monotremata, Ornithorhyncus has a smooth brain while that of Echidua is convoluted. After considering the orders in which the pallium is slightly convoluted our author notes the order of appearance of the fundamental fissures in these forms and shows that, while the sylvian fossa is to be associated with the rhinencephalon, the sylvian fissure belongs to the pallium. Further among the lissencephala—or smooth-brained forms—the sylvian fissure is by no means necessarily the first to appear. In many forms there is a tendency to the formation of a sagittal fissure and marginal gyrus before the sylvian can be recognized. Taking the fundamental fissures it is evident that there is no fixed order for their appearance but that the order differs according to the groups of animals examined.

For purposes of general description Turner groups the fissures into sagittal, arcuate and radial, terms which hardly require further explanation.

Taking up first those orders in which the convolutions are most complex, it appears that the representatives of smallest size in which the brain is large as compared to the body, may be lissencephalous, as for example in the case of little marmoset monkey among the Primates. In the carnivora, pinnipedia, cetacea and ungulata certain accurate fissures arch over the sylvian—itself to be classed with the radial fissures—and in the most typical cases form three concentric gyri, which, enumerated from the sylvian fissure outwards, are the sylvian, suprasylvian and marginal gyri. Where it is deep, the sylvian fissure always hides the Insula.

In the carnivorous brain the crucial fissure is a characteristic feature extending from the mesal surface outwards and bounded at its lateral end by the sigmoid gyrus. The mesal surface in this group has a well marked splemal fissure—both longitudinal and accurate in its course, having important relations to the crucial fissure just named.

The homology of the fissure of Rolando with the various fissures in the carnivorous brain with which it has been compared are merely mentioned and the author passes on to propose the question whether there can not be an occipital lobe without a parieto-occipital fissure and decides that there can be if the caudal prolongation of the lateral horn, the post. cornu, is taken as the criterion. Again he argues for the recognition of both frontal and parietal lobes even where the fissure of Rolando is absent. It is plain from what has been said that the convolutions can have very little value in determining phylogenetic relationship and that their significance is not fundamental. The remaining pages are devoted to the various theories of the formation of the convolutions. This is the least satisfactory portion of the paper. It should be added that there are more than forty cuts interpolated in the text, many of them representing the brains of unusual or rare animals.

II.—ASSOCIATION, REACTION.

PROF. J. MCK. CATTELL,
Columbia College.

HÖFFDING. *Ueber Wiederkennen, Association und psychische Activität.*, Viertelj. f. wissensch. Philos. 1889 XIII 420; 1890 XIV 27, 167, 293.

LEHMANN, *Kritische und experimentelle Studien über das Wiedererkennen*, Phil. Stud. 1891 VII 169.

WUNDT, *Bemerkungen zur Associationslehre*, Phil. Stud. 1891 VII 329.

JASTROW, *A statistical study of memory and association*, Educational Review, 1891 II 442.

RIBOT, *Enquête sur les idées générales*, Rev. Philos. 1891 XXXII 376.

MUENSTERBERG, *Zur Individualpsychologie*, Centralbl. f. Nervheilkunde, 1891 XIV 196.

DUMAS, *L'Association des idées dans les passions*, Rev. Philos. 1891 XXXI 483.

The papers by Prof. Höffding, Dr. Lehmann and Prof. Wundt are intimately related to each other, as well as to preceding articles and text-books. Höffding criticises Lehmann's previous paper "Ueber Wiedererkennen" (Phil. Stud. 1888, V 96-156), in which the latter maintained that it is not necessary to assume similarity as a principle of association. Höffding argues for the integrity of association by similarity, laying special stress on the recognition of previous experiences. In such cases the recognition is the psychological correlate of the greater mobility of the corresponding molecules of the brain. A change which has once taken place occurs the more readily the second time. Lehmann argues that every experience is complex, and that the recognition, even of a comparatively simple sensation, is due to contiguity rather than similarity. Wundt in view of these papers and of Scripture's recent experimental study explains and elaborates the doctrine of association contained in the third edition of his psychology. He holds that simultaneous association should be ranked co-ordinate with successive association, and that the latter depends, as the name itself indicates, on the continuous interweaving of all the ideas under the control of conciousness. We may look on the disagreement of our leading psychologists in these questions without great anxiety, for after all the matter is largely one of nomenclature.

Turning to the experimental results of Lehmann's paper, we find them to be of interest. In his first section he gives the results of 428 trials on the recognition of smells. In 45% of the trials an immediate association was called up, in ¼ of which the observer was apparently mistaken as to the nature of the smell. In 28% of the trials a name was immediately suggested, which was wrong ¼ of the time. In 7% of the trials the smell was recognized, but called up no association nor name, and this class Lehmann considers the most interesting theoretically. The second section is on the recognition of sounds. Lehmann finds, as Starke and Merkel had previously found, that the intensity of the second of two sounds is overestimated. Lehmann thinks that this is because the actual sensation must appear stronger than a memory-image. On this assumption the longer the interval between the two sounds, and the less exact the memory-image of the first sound, the greater should be the overestimation of the second sound. The experiments given in support of this theory do not seem to confirm it very well, but Lehmann thinks the variations are due to a periodicity in the fading of the memory-image. It is not, however, a matter of course that because a sensation is remembered less exactly, it should be represented as weaker. The complex effects of memory, contrast and fatigue cannot be satisfactorily explained on this theory. The writer of this notice finds that the second of two weights is, indeed, overestimated, but the second of two lights is still more regularly underestimated. It may be suggested that it would be more convenient and accurate if writers who know mathematics (as Lehmann does) would give probable errors and not merely the number of mistakes made in 60 or 1380 trials.

Prof. Jastrow publishes the results obtained with his classes in the University of Wisconsin and on students in the Milwaukee High School. He gave ten words separately to the students, and obtained the first associated words. After 48 hours he required them to write down as many of the original words as they could remember. Then he gave them the original words, and required them to write down as many of their former associations as they could remember. He obtained the interesting result that, while about ½ of the words were forgotten, only ⅕ of the associations were forgotten. Some of the words were remembered much better than others, whereas the associations on the several words were remembered about equally well. In the classification of the associations the great frequency of certain associations (pen-ink, cat-dog, etc.,) is apparent. There were only 241 different associations in 700 cases. About ½ the associations come under the heading "natural kind, or one object suggesting another of the same class." After this division "whole to part" was the largest. The results obtained with the university students and with the school students were much alike. The women remembered better than the men, and their associations were the less diversified. This illustrates an important distinction, which obtains throughout the animal kingdom—the greater variability of the male.

Prof. Ribot gave words to 103 persons, and recorded the suggested ideas. "Nothing" was suggested 53% of the whole number of times! The observers were classified according to the nature of their mental imagery. The visual type in which a more or less distinct image of the object is called up was the most frequent. The types in which printed words were seen, or auditory images prevailed, were rare. Prof. Ribot formerly called attention to the importance of movement to imagination, but in the present paper nothing is said concerning those whose thoughts are chiefly accompanied by the impulse to spoken words or other movements, to which class Prof. Stricker and the writer of this notice belong.

Dr. Münsterberg has made tests concerning association and various mental traits in school children and others. In the present paper he describes the methods he has used, but does not as yet give his results. The tests suggested by Münsterberg need not be described here until their appropriateness has been demonstrated by the publication of these. But one cannot fail to honour the heroic perserverence which is borne witness to by experiments of this sort.

The paper by M. Dumas does not contain experimental results.

MARTIUS, *Ueber die Reactionszeit und Perceptionsdauer der Klänge*, Phil. Stud. 1890 VI 394.

MARTIUS, *Ueber den Einfluss der Intensität der Reize auf die Reactionszeit*, Phil. Stud. 1891 VII 469.

Dr. Martius here continues the publication of careful experiments on reaction-time carried out in his private laboratory at Bonn. In his first paper he gives experiments showing that the reaction-time becomes shorter as the pitch of a tone is taken higher. A monochord was used to produce the tones, and the times were measured with the Hipp chronoscope. C^{I}, c^{I}, c^{II}, c^{III} and c^{IV} were used, and the times compared with those obtained from the noise made by a hammer and anvil. In a general way the times are the same (in the neighborhood of 110σ) for c^{IV} as for the noise, and about 40σ larger for C^{I}. There are considerable differences with the three observers, which are probably due to the limited number of experiments, 12 to 19 of each sort, with an average variation of about 10σ. Martius concludes from a comparison of the reaction-times that 1 to 4 vibrations are sufficient to call up a sensation.

Prof. Stumpf in reviewing this paper (Zeitsch. f. Psych. II, 230-232) suggested that the difference in time of the reaction might be due to the greater intensity of the higher tone. Martius consequently made experiments in which the intensity was varied, and obtained as result that there is no difference in the length of the reaction-time for sounds of different intensities. This is contrary to the results obtained for several classes of stimuli by Wundt, Exner, v. Kries u. Auerbach, v. Vintschgau u. Honigschmied, v. Wittisch, Berger and the writer of this notice. Martius thinks that this discordance is explained by the greater attention given in his experiments, but it more likely due to the small range of intensity. The intensity of the sounds was not measured, but in no case can a monochord give a very loud sound.

REPSOLD, *Neuer Vorschlag zur Vermeidung des persönlichen Zeit-Fehlers bei Durchgangsbeobachtungen*, Astronomische Nachrichtungen 1889 Dec. 9, No. 2940.

BECKER, *Ueber einige Versuche von Durchgangsbeobachtungen nach dem neuen Repsold'schen Verfahren*, Astronomische Nachrichten 1891 May 19, No. 3036.

LANDERER, *Sur l'équation personelle*, Comptes rend. 1889 CVIII 21.

GONNESIAT, *Sur l'équation personelle dans les observations de passages*, Comptes rend. 1891 CXII 207.

STROOBANT, *Recherches expérimentales sur l'équation personelle dans les observations de passage*, Comptes rend. 1891 CXIII 457.

ANDRÉ ET GONNESIAT, *Etude expérimentale de l'équation décimale dans les observations de passage, faite à l'Observatoire de Lyon*, Comptes rend. 1892 CXIV 157.

CHRISTIE, *Change of personal equation with stellar magnitudes in transits*, Monthly Notices of the Royal Astron. Soc. 1891 455.

BACKHUYZEN, *Bestimmungen der persönlichen Gleichung bei Passagebeobachtungen*, Viertelj. d. Astron. Gesellsch. 1889 249.

A personal equation machine, The Sidereal Messenger 1891 139.

The photochronograph and its application to the star transits, Georgetown College Observatory 1891 36.

Prof. Wundt and Prof. Exner have called attention to the psychological interest of the personal equation long known to astronomers, and

Dr. Sanford has given in this JOURNAL a thorough historical and critical review of the subject up to 1888. Since then astronomers have continued the study of a subject so essential to their science. The accuracy with which time can be measured, and with which position and motion can be determined, is dependent on the personal equation, and reduction of the error by .01 sec. would be an important advance in astronomy. The papers by Dr. Repsold and by Prof. Becker suggest a new method for eliminating or lessening the personal equation. Repsold had previously proposed that the transit instrument might be moved at the same rate as the star, and the observer might at his leisure adjust the wire so as to bisect the star. If the position of the instrument were known, the time of transit could be measured very exactly. But the mechanical difficulties of moving and adjusting the instrument proved insurmountable, and Repsold now proposes that the wire only should be moved by the observer. This is done by means of a screw, and the position of the wire is registered automatically on the chronograph. A number of such registrations can be taken at short intervals during a single transit. Repsold tested his method artificially, and found the variable error of observation to be comparatively small (44σ to 27σ), and the constant error to be nearly eliminated. Becker tested the method in actual transit observations with less satisfactory results. The personal differences of four observers were obtained by the chronographic method and by Repsold's method, and the results were reduced in the usual manner. The probable error was found to be one-fourth to one-fifth larger by Repsold's method. It seems likely, however, that improvements in the apparatus and practice on the part of the observers would make this new method the most accurate hitherto used.

Of the reports in the *Comptes rendus* that by M. Stroobant has the most psychological interest. He calls attention to the fact, familiar to psychologists, but not systematically applied in astronomy, that an observer can judge when his registration is worse than usual. The writer of this notice finds in recent experiments that the error in adjusting a movement is about half as great as the error in perceiving it. The error in adjustment is perceived as an error and may be eliminated, the total error being thus reduced by about one-eighth. Stroobant finds that the "eye and ear" method is not much less accurate than the chronographic method. The first limb of a planet (artificial) was registered too soon (115σ to 238σ), and the second limb too late (16σ to 52σ). Registrations become later as a sitting is continued. Stroobant finds that he has a considerable decimal equation, and this error is more elaborately studied by MM. André and Gonnessiat. They find it to be about .05 sec. The decimal equation (first noticed by Prof. B. Pierce), is due to giving preference to certain decimals in estimating parts of seconds or millimetres. It would seem to be an error difficult to eliminate. Observers differ greatly, and an observer's knowledge of his own error would probably lead to its alteration. M. Landerer discusses the part which diplopia (doubling of the image) may play in transit observations. This defect can of course be corrected by glasses, but it may be increased by fatigue during the observation. The British astronomer royal gives the alteration in the personal equation with a change in the brightness of the artificial star. The star was darkened by gauze netting, but the alteration in intensity was not measured. The registration tended to become later, as the star was made less bright.

Prof. Backhuyzen explains his method for studying artificial transits, which is described in detail in Vol. VII of the annals of the Leiden Observatory. In this method the artificial star is stationary, and the apparent motion is obtained by a revolving prism. The Eastman personal equation apparatus is described anew in the *Sidereal Messenger*. This is the only artificial transit apparatus which the writer has examined and he does not know how it compares with others. But it is one of the most recent and considered one of the best. The technical advances in psychology are borne witness to by the fact that the psychologist would not like to use a recording apparatus whose error is over 1σ, whereas the variable error of this instrument is about 20σ, and the constant error is not entirely eliminated by the ingenious method of reversing the motion of the carriage.

The paper last on this list is the most important for astronomy, but does not especially concern psychology. As long ago as 1849, Faye suggested the possibility of recording transits by photography, and this has now been actually accomplished. It is not necessary to describe here the methods and apparatus used in the Georgetown College Observatory under the general direction of the Rev. Father Hagen. Stars of the fourth magnitude have been successfully photographed in transit, what Prof. Young calls the "annoying human element" being largely eliminated. The photographic method will probably be applied with great advantage to many physical measurements.

The interests of psychology are not especially served by any of these papers. Astronomers naturally wish to do away with the personal equation rather than to study it. The most important advances have been in this direction. The Repsold method transfers the error to a certain extent from the observer to the instrument, and the photographic method does away with the observer altogether during the actual transit. The work of astronomers becomes less important for psychology as their devices become more mechanical, and as psychology itself learns to state and to solve its own problems. On the other hand recent advances in psychology are of increasing importance to astronomy and the other physical sciences. Physical measurements in the last resort must always depend on the accuracy of the eye and hand. Errors of observation are now studied in psychology with an exactness which has never been approached in any physical science. There are but few physicists and mathematicans who understand the position of psychology in this matter. The physicist cannot know the true value of the quantity he is seeking to determine; he deals with residuals not with errors. The psychologist on the other hand determines actual errors, and can study their nature, size and dispersion in a manner entirely beyond the reach of physics. The whole theory of the method of least squares is concerned with variable errors, and is helpless in the presence of constant or systematic errors. Constant errors are, however, far more important and dangerous than variable errors, and these can be measured and eliminated by the psychologist. Astronomers have, indeed, attempted this with their artificial transit instruments, but they have been playing the part of the psychologist, in most cases without adequate methods or knowledge.

ARDIGÒ, *Alcune osservazioni relative alla legge psicologica del riconoscimento*, Rivista di filosofia scientifica 1891 X 577.

The author relates an experience in the reproduction of a dream which seems to support his theory of re-cognition. He presents several considerations on cognate points from which he deduces two consequences, the one in regard to the association of ideas and the other relation to the theory of reasoning. The former denies that the process of association is the revival of terms one after the other that exist separately in the organic predisposition of the cerebrum, but asserts that it is a reenlivement little by little in various parts of an ample system which acting in its integrity from one point to the other does so in successive moments and with variations of intensity in different parts. Thus it is deduced that the principle of association is the same for simultaneous and successive associations and for those of similarity; moreover we see that there are two species of simultaneous and successive associations, the direct and the indirect. The direct association of coexistence and sequence takes place because the single system preformed as a physiological synergy is aroused in its integrity, reacting successively in its parts. The association by similarity takes place because the special rhythm of such an entire system stimulates analogous rhythms of other systems physiologically preformed, in the same way that a piano-string in vibration produces resonating vibrations in other strings of analogous rhythm. The indirect association of coexistence and sequence takes place because the rhythm of activity, when there is consciousness of one term of an associative series, arouses the analogous rhythm of a term of another associative series in such a way that the whole physiological system takes part in it. Reasoning is nothing more than a product of the law of re-cognition.

E. W. SCRIPTURE.

SCRIPTURE, *Ueber den associativen Verlauf der Vorstellungen*, Inaug. Diss. Leipzig 1891; also Phil. Stud. 1891-2 VII 50.

The first step to a scientific treatment of the subject must be a careful collection of material instead of the fictitious examples generally in use. The course of ideas in consciousness can for the sake of scientific study be divided into four processes: preparation, influence, addition and posterior effect. The process of preparation is the change which an apperceived idea undergoes before it influences the course of consciousness. In one form of association the whole of the apperceived idea acts and remains in the result; e. g. the word Kothe calls up the phrase "in Kothe," (p. 17). In another form the whole of it evidently acts but the resulting idea does not contain it: e. g. touch-impression from a piece of paper—word "paper," (p. 17). Often only part of the apperceived idea is of effect, that is, it is diminished by the concentration of the attention on certain parts which are active in producing the result whereas the other parts are apparently lost; this is the process of the diminution of an idea. Example, Rahm—Raum; the association is caused by the three letters while the other disappears (p. 20). The second fundamental process is the influence of ideas on the course of consciousness. It is of two kinds, direct and indirect; the former is the case where an idea produces a change without the intervention of another idea; example, "ach!"—"ach, weh!" taste of lemon juice—word, "lemon juice;" sound of a tuning-fork—visual image of a tuning-fork (p. 26). The other form is the indirect influence. Sir Wm. Hamilton thinking of Ben Lomond associated to it the apparently unconnected Prussian system of education; he had, however, once met a German on that mountain and the association can be explained by supposing the unconscious links of association thus: Ben Lomond—the German—Germany—Prussia—the Prussian system. To test the point by experiment, a series of cards was prepared on half of which were German words, *A*, *B*, *C*, *D*, and some unknown Japanese letters, u, v, w, x; the other half contained Japanese words in Roman characters, *M*, *N*, *O*, *P*, with the same Japanese letters, w, u, x, v. The series having been shown in this way, one of the German words was then exposed without the Japanese letter and the observer was to notice on what he next thought. The Japanese letters were generally forgotten and the Japanese word in Roman characters was often associated without the observer knowing why. The probability of the correct Japanese word being associated to the German word was about one to five; actually this occurred in the ratio of nearly three to two, or, if some cases where other influences were at work be omitted, in the ratio of two to one. Experiments with other combinations of ideas, e. g.

words, colors, names, etc., seldom give such results, the direct influence being generally the more powerful. The forgotten or semi-forgotten Japanese letters were to be found in various degrees of consciousness, and several pages are devoted to an investigation of them. The third process, addition, can be illustrated by the following examples: The—Thee; sound of two pieces of wood rubbed together—visual image of the small pepper-boxes (which grind) at table in a Swiss hotel (p. 44). The addition of elements to an idea often takes place while the idea itself undergoes a diminution as above described; this may go so far that none of the original idea is left, every substitution is thus an addition with diminution. A large collection of examples is given illustrating the various forms of the process. One of the most interesting points is the addition of the coefficient of recognition (first noticed by Höffding); the simplest form is seen in the example: touch-impression from a piece of silk—recognition of an indefinite touch-impression, (p. 57). The development of the quality of recognition into localization in space and time is illustrated by numerous examples. The Herbartian revival of ideas and the English reproduction of ideas are impossible terms, ideas being neither revived nor reproduced; the facts are limited to the existance of an idea at a given moment which exhibits certain properties that we attribute to previous occurences in consciousness. These properties are called after-effects. One peculiar case is experimentally investigated, namely, the effect of an unperceived element. A series of picture cards is shown with such short exposures that only the picture is seen while a letter in the indirect field of vision entirely escapes notice. Then the letters are shown singly and the observer is asked to say what picture belongs to each of them. The results show that the unapperceived portions of an idea are sufficient to call up the idea. The bearing of these experiments and those on indirect influence in explaining cases of apparently disconnected successions of ideas is evident. AUTHOR.

III.—HYPNOTISM AND SUGGESTION.

PROF. J. JASTROW,
University of Wisconsin.

BÉRILLON, *Les faux témoignages suggérés chez les enfants*, Rev. de l'Hypnotisme 1892 VI 203.

Dr. Bérillon recounts some observations on children both in the waking and hypnotic conditions. He asks a child to pay especial attention to his words and says: "you will forget your name;" the child is really unable to speak its name, although evidently struggling to do so. Another boy ten years of age is asked to tell what he did the day before, he mentions that a Mr. J. was present at dinner, when he is interrupted with the statement that his memory is confused and that he doesn't know whether Mr. J. was present at lunch or at dinner. His mother asks him to remember but the recollection is gone. Another boy 12 years of age is told that when in the street yesterday he saw two men fighting, the one struck the other and fled. The man was large and so on. Upon questioning the lad recalls the whole scene and will not believe it was suggested to him. Another child similarly is made to accuse a respectable neighbor of theft, or accuse his schoolmate of assaulting him and so on. The suggestions are often extended by the immagination of the subject. Dr. Bérillon concludes that with children from six to fifteen years of age it is easy by simple affirmation either in the waking condition or in hypnotism. to promote illusions of perception, partial amnesia, distortions of memory and hallucinations. The realization of such suggestions in children, is the rule and the failure the exception. The readiness with which these phenomena may be utilized for inducing false testimony is obvious and should be taken into account in all legal cases in which the testimony of children is admitted. As evidence of the sincerity of the children Dr. Bérillon offers his own impressions, and the fact that in many cases the suggestions were realized at the first meeting and when the children were in ignorance of the expected result. They were selected from all classes of the population, and Dr. Bérillon is of the opinion that the intelligent children are more rather than less susceptible to its influences. The great suggestibility of the children seems clearly related to the great prevalence of good hypnotic subjects in France, and it may be questioned whether a similar condition of suggestibility would be found amongst the children of our own country.

GUÉRIN, *Considérations juridiques à propos des faux témoignages suggérés*, Revue de l'Hypnotisme 1892 VI 212.

The French code punishes false testimony with the same penalty that attaches to the accused if convicted by such testimony, and although the testimony of children is under special regulations, the possibility of injustice by suggested testimony is not diminished thereby. Just as the inebriate is responsible for the effects of his passion when he first indulges it so the subject of suggestion is responsible for allowing himself to be the subject of suggestion. "He is as culpable for accepting criminal suggestions as he would be for following bad advice; the situation is the same." The danger for the accused is extreme, and it is the business of the students of hypnotism to furnish means whereby the suggested may be distinguished from the true experiene and whereby the author of the suggestion may be discovered. Equally important is the necessity of limiting these practices to physicians and allied scientists.

VOISIN, *Délit de vol commis sous l'influence de la suggestion hypnotique*, Revue de l'Hypnotisme 1892 VI 219.

A woman aged twenty, subject to hystero-epilepsy, catalepsy and somnambulism was arrested for stealing many objects from the Magasins du Louvre. It appeared that for three months she had been stealing with extreme adroitness at the suggestion of some accomplices. At the same time her suggestibility in the waking condition was so great that her companions could make her do and believe almost anything. On recommendation she was sent to the Salpêtrière instead of to prison and was there restored mainly by suggestion.

GOIX, *Anorexie hystérique traitée avec succès par la suggestion hypnotique*, Revue de l'Hypnotisme 1892 VI 245.

Anorexia is the persistent refusal of food and may result fatally; the sole cause is that the patient does not want to eat. Marie Ch..... aged 23 appears September 10 before Dr. Goix and has not eaten for four days; during this time her energy and industry are extraordinary. Hysterical symptoms are clear. She is hypnotised but refused to promise while hypnotised, that she will eat. The next day while hypnotized she drank a cup of chocolate which she is told is water, (water she takes at all times), but still refuses the suggestion of eating. Still later the suggestion is given her that she will repeatedly say "I will eat, I will eat." By repeated suggestion, setting the time of eating, threatening her with severe pain, the opposition is at last broken down and a normal appetite ensues. The case is offered as showing the importance of the precise formula of suggestion, the need of special adaptability to each case and the possibility of administering food during the hypnotic condition.

ARTIGALAS *et* RÉMOND, *Note sur un cas d'hemorrhargies auriculaires, oculaires et palmaires, provoquées par suggestion*, Revue de l'Hypnotisme 1892 VI 250.

The patient, Mme. F., aged 22 years, after an operation (uterine tumor) quite unexpectedly manifested hysterical symptoms. The most remarkable of these was the shedding of tears of blood. Hypnotic suggestion at first failed to stop these, while it was quite sufficient to say "you will bleed in a minute" to promote the phenomena. Again hypnotized it was suggested that she should bleed in the palm of her left hand. A bloody perspiration followed in a few minutes. Then it was suggested that the bleeding would stop at the palm and also at the eyes, and in this way she was speedily cured. The case is naturally brought into relation with the cases of stigmatisation and it is easy to see that in combination with a religious order this symptom might have been given a mystical significance.

MACDONALD, *Traumatic Hypnotism*, Science 1892 XIX 23.

The account tells of a physician who was thrown out of her cart and suffered a contusion on the right parietal protuberance over the third descending convolution. The last thing the patient remembered was calling to a man to get out of the way. The report of others shows that after the accident she said she was not hurt, washed her face and hands, gave directions and answered questions. For a moment she awakes but relapses into this condition again, delirium also ensues. Mr. MacDonald regards this as a case of traumatic hypnotism.

WRIGHT, *Traumatic Hypnotism*, Science 1892 XIX 66.

Describes the case of a boy thrown off a horse against a barn door, who thereupon arose, finished his farm duties, went to the house, took a light supper and answered questions; he seemed entirely normal except for a vacant start and an occasional senseless laugh. He went to bed and on awakening next morning was found to have no memory of anything after the accident. The author regards this as a case of spontaneous hypnotism, differing from the preceding one in that the patient does not pay any attention to the accident but goes on automatically with his routine work.

BALDWIN, *Suggestion in infancy*, Science 1891 XVII 113.

Do ideo-motor or suggestive re-actions, have any part of normal mental life or is the hypnotic sleep to which this may be affiliated essentially artificial? As a contribution to this inquiry Prof. Baldwin utilized his observations of his child during her first year. If ideo-motor suggestions are normal then early child life should present the most striking analogies to the hypnotic state in this respect. Three kinds of suggestions are distinguished: 1. physiological, 2. sensori-motor, 3. ideo-motor; this being the order in which they appear in child-life. 1. The meaning of physiological suggestion is sufficiently shown by one of the observations. For the first month or six weeks the life of the child is mainly physiological, the vacancy of consciousness as regards anything not immediately given as pleasure or pain precludes the possibility of ideal suggestion as such; no ideas in the sense of distinct memory-images are present. Yet suggestions of sleep began to tell on the child before the end of the second month.

She was put to sleep by being laid face-down and patted. This soon became not only suggestive of sleep but also an indispensable suggestion. 2. Among the sensori-motor suggestions we find various sleep suggestions, food and clothing-suggestions and suggestions of personality. For the next month there was an increasing power of the sleep suggestion just mentioned. In the mean time two nursery rhymes were added. In the third month a difference was noticed between the effect of the suggestions coming from the nurse and those from another person. In the fourth month the father succeeded with difficulty in substituting his suggestions for those of the nurse although they were imitated with the greatest care. The sleep suggestion thus depended on the personality of the nurse—the peculiar voice, touch, etc. The power of the father was gradually developed, succeeding at night better than in the day-time; darkness was thus an additional suggestion. A single flash of bright light causing a closure of the eyes was often a most powerful suggestion. At this time other persons had great difficulty in producing sleep, whereas the father succeeded in a short time. At the end of a year the child would voluntarily throw herself into position at a word and would go to sleep, if patted, in from four to ten minutes. At 16 months even when the nurse is unable to do anything with her the mere sight of the father makes her quiet and in five minutes put her to sleep. This illustrates the passage of a purely physiological suggestion into a sensory one. The sight of the rubber on the end of the food-bottle—not the bottle alone—was suggestive of movements as early as the fourth month; the touch of the bottle with the hands was not suggestive till later. At the fifth month the sight of mittens, hood and cloak caused signs of joy. (The referee has noticed a case at the same age where the sight of the mother with a bonnet on at once produces quiet when the child is restless, the restlessness returning if the mother departs; whereas the same does not occur if no bonnet is worn.) The ideo-motor suggestions are of two kinds: deliberative and imitative. By deliberative suggestion is meant a state of mind in which co-ordinate stimuli meet, affront, oppose, further, one another. A most instructive case is reported showing the conflict between the impulse to scratch and the idea of the punishment, the latter gradually overcoming the former. Imitative suggestion is of two kinds: simple and persistent. Illustrations of these will at once occur to the reader. In conclusion the facts of suggestion as stated from the nervous side are as follows: Physiological suggestion is the tendency of a reflex to get itself associated with and influenced by other sensory or ideal processes; sensori-motor suggestion is the tendency of all nervous re-actions to become secondary-automatic and reflex; deliberative suggestion is the tendency of different competing sensory processes to merge in a single motor re-action, illustrating the principles of nervous summation and arrest; persistent imitative suggestion is the tendency of a sensory process to maintain itself by such an adaptation of its re-actions as to transform them into new stimulations. From the side of consciousness, suggestion in general is the tendency of a sensory or ideal state to be followed by a motor state.

E. W. SCRIPTURE.

Studies in hypnotism at Brown, The Brown Magazine 1891 III 1.

In the course of some experiments on hypnotism at Brown University two cases occur that are of interest. The first shows the resistance of the subject to post-hypnotic suggestion and his way of avoiding a seemingly ridiculous action. The subject was told that on waking he should say *ee* instead of *ä*, as "feether" instead of "father." When awakened he was asked: "Is one of your parents living?" "Yes, sir." "Your mother?" "Yes, my mother and—and—" he apparently tried to say "father," smiled and added "and—both of them." "But you were about to say your mother and—?" "My mother and—and—her husband." The second case is that rare occurrence, auto-hypnotism. One of the students can sit down, lay out a certain course of action, hypnotize himself, performed the predetermined operations, return to his seat and wake up. While in this state, no outside personality has any influence over him. He has used this power several times to induce sleep at night, waking as usual the next morning. On one occasion feeling rather exhausted he dropped into a chair and said he would hypnotize himself, in order to feel well upon awakening. Accordingly he did so, and after about forty-five seconds awoke declaring that his head felt much better, though his body was still tired. It is to be regretted that the observations were conducted for their popular interest more than for their scientific value; it is to be hoped that this case of auto-hypnotism will be more carefully observed and described.

E. W. SCRIPTURE.

IV.—SIGHT.

GREEFF, *Untersuchungen über binokulares Sehen mit Anwendung des Heringschen Fallversuchs*, Zeitschrift fur Psychologie and Physiologie der Sinnesorgane, 1892 III 21.

This is a more careful test of Hering's experiment with the falling balls in order to determine the accuracy of our perception of the third dimension and the conditions upon which it depends. The apparatus used by Hering and the conditions of the experiment were somewhat modified by Dr. Greeff, but only with a view to greater mathematical accuracy in the results, and to a greater variation of the circumstances under which the judgment of observer was to be formed. The distance between the eyes and the point of fixation was made definite and measured. A screen was employed so as to make the angle at which the falling ball could be seen the same for all the experiments. Also a perforated screen was placed above the line of vision with the holes in it at regular distances which were measured. The balls were dropped through these perforations and the judgments of the observer recorded with the known and definite distance of the falling ball from the point of fixation whether before or behind it. The design in shutting off from view a part of the distance of the falling ball and including only that came within the limits of a given angle was to exclude the influence of occular movements upon the judgments of localization in relation to the point of fixation. The observer looked through a conical shaped roll of paper with the inner surface darkened, and the apex or smaller end farther from the eye in order to prevent the entrance of disturbing rays of light into the eyes. This conical tube was about 30 cm. long, and the wider end about 20 cm. wide. The point of fixation in a box of 60 cm. length and 20 cm. width was situated 95 cm. from the eyes. This distance, however, seemed to vary with the conditions necessary to produce the parallel position of the eyes by means of a prism before one of the eyes. This expedient was resorted to in order to remove the force of the supposition that a convergent position of the eyes had something to do with the judgments of localization. In all his experiments Dr. Greeff found that at all distances the judgment of distance was as correct when the eyes were in a parallel position as when convergent. The observer's confidence and certainty were as great in one case as in the other. The 2 to 3 per cent. of failures he attributes to the fluctuations of attention and the coincidence of winking with the fall of the ball. The most noticeable feature is the marked difference between monocular and binocular vision in regard to the correctness of the judgment of distance. The first set of experiments represents four different conditions with a common point of fixation 1.12 cm. from the observer. The first 50 trials were with monocular and the second 50 with binocular vision and without the use of prisms. The third set was with the aid of a prism to produce the parallel position of the eyes, and the fourth set of 60 trials was with prisms that produced a slightly divergent position of the visual axes. The judgments represent the relative localization of the falling ball compared with the point of fixation. Following are the results:

	No. of Balls.	Right Judgments.	False Judgments.
Monocular vision,	50	26	24
Binocular vision, free,	50	49	1
Binocular vision, with parallel axes; Prism 7,	50	50	—
Binocular vision, with divergent axis; Prism 8,	20	18	2
Prism 10,	20	20	—
Prism 12,	20	17	3
Prism 14,	20	8	12

The prisms were placed before the right eye and were multiplied in order to produce the utmost degree of divergence possible. The results are three-fold. *First*, the increased accuracy of binocular over monocular vision. *Second*, the equal accuracy of all three positions of the eyes in the judgment of relative distance; namely, the convergent, parallel, and divergent positions. *Third*, the coincidence of error with that degree of divergence which overcomes the tendency to fusion. When the same general principles were observed and the point of fixation was made 60 cm., of 200 falls in monocular vision 51 per cent. was false, and 200 in binocular vision only 1.5 per cent., 98.5 per cent. being correct. This set of conditions was assumed as a starting point for increasing the distance of the point of fixation in order to see what the limit of correct localization would be when the balls fell the same distance before or behind the point of fixation. When the last was made 1 m. the percentage of failures was still 1.5 per cent; at 1½ m. it was only 1 per cent. At 2 m. the failures increased to 3 per cent. and at 3 m. they were 6 per cent. This result suggested two changes in the experiment. First, the enlargement of the angle through which the ball fell; and, second, the increase of the distance from the point of fixation for the falling ball. It was found that with the enlargement of the angle of vision and thus of the visual field under the same conditions as above the failures decreased down to 2.5 per cent. when the distance between the edges of the screen was 20 cm. This was for binocular vision. For monocular vision under these last conditions the errors were 4.3 per cent. But with the fixation point 3 m. distant and starting with 1 cm. distance from that point for the falling ball this distance was increased up to 10 cm. when the percentage of errors had fallen to 2 per cent. from 6 per cent. This relation was more particularly determined by a set of experiments with two different persons and the same general result obtained. The general conclusion reached by Dr. Greeff was that the localization was proportioned in its definiteness and accuracy to the ratio between distance of the fixation point from the eyes and the distance of the falling ball from that point. The matter was then more carefully tested by the study of cases in which one of the eyes was either naturally or artificially affected by influences that diminished the distinctness of the images: naturally by maculae corneae, cataracta incipiens, amblyopia congenita, etc.; artificially by powdered lenses.

The results in localization were much as in the first case. The number of errors was remarkably small, being for 100 trials 2 per cent. when the fixation point was 1 m., 3 per cent. when it was 2 m., 5 per cent. when it was 3 m., 18 per cent. when it was 4 m., and 39 per cent. when it was 5 m., and the left eye representing a distinctness of $\frac{6}{12}$ to $\frac{6}{6}$ for the right eye. With the ratio of $\frac{6}{6}$ for the right and $\frac{1}{36}$ for the left eye, and the distance of the fixation point 1 m. the errors were 3 per cent., and 20 per cent. when the distance of the fixation point was 2 m. The distinctness of vision, therefore, according to Dr. Greeff, has very little to do, within moderate limits, with the perception of relative distances.

Columbia College. J. H. HYSLOP.

HERING, *Prüfung der sogenannten Farbendreiecke mit Hülfe des Farbensinnes excentrischer Netzhautstellen*, Archiv. f. d. ges. Physiologie 1890 XLVIII 417.

At the time of the publication of Hess's study of the peripheral color sense Hering based upon it a critique of the Young-Helmholz theory (both papers reviewed in this JOURNAL, III, 203, 204). The present paper is a continuation of that critique with particular reference to the color-triangle of König and incidentally to those of Maxwell and Fick. Hering finds one and all of them and indeed all possible color-triangles made upon the Young-Helmholz theory out of harmony with the facts established by Hess. The discussion is technical and for it the reader is referred to the original. In the latter part of the paper he also shows the irreconcilable opposition between the observed brightness of colors seen with the periphery of the retina and the three-color theory, citing in part the results of a study of that subject, also by Hess, presently to be published. The reply of Fick to his former paper, reviewed in the JOURNAL, III, 574, reached him to late for special rejoinder, but he considers its points answered in the present paper, and counts upon Fick as a convert when he shall have investigated the matter by Hess's method.

E. C. SANFORD.

KLOBUKOW, *Vorlesungsversuch zur Demonstration der Wirkung von Complementärfarben und Farbengemischen beim Zusammenbringen von gelösten Farbstoffen*, Ann. d. Physik u. Chemie 1891 XLIII 438.

In explaining the effects of mixing colors to a large audience it is very desirable to have direct mixtures and not those produced by the color-discs. It is proposed to have colored solutions of the desired shades so prepared that they are not soluble in and cannot take color from one another but have a great difference of specific gravity. Two solutions are shaken together in such quantities that the desired color is produced. The mixture is than allowed to stand a short time, at the end of which the two component colors are found separated one above the other. For example, to show the effects of a mixture of red and green a solution of aldehydegreen in amylalcohol and one of cobalt-salts in water are used. If the proper strengths are employed the mixture is a dirty white. The addition of common salt to the cobalt solution hastens the separation of the two. For mixtures of blue and yellow a solution of phenanthrenchinon or some other derivative of chinon that is insoluble in water but soluble in amylalcohol and an ammoniacal solution of copper in water are to be used. The mixture is a bright green and serves to show that mixtures of pigment colors are different from those of spectral colors. Likewise a solution of chinon in amylalcohol and a combination of the solutions of cobalt and copper in water (as near as possible to the violet of the spectrum) will give a dirty white when mixed in the proper proportions, whereas the corresponding spectral colors are not complementary.

E. W. SCRIPTURE.

OTTOLENGHI, *Nuove osservazioni sul campo visivo in psicopatici*, Archivio di Psichiatria, Scienze Penali ed Antropologia criminale, 1891 XII 112.

Ottolenghi's complete memoir of part of which a résumé is here given will appear as Vol. IX of the *Biblioteca Antropologica Giuridica*. The cases examined, numbering about 60, include criminals, neurasthenics, hypnotic subjects, epileptics and prostitutes. The numbers are too small to base general theories upon, but some very interesting facts were brought out by investigations. In the case of criminals the irregularity of the field of vision,—more or less accentuated limitation, and vertical hemiopia,—was found to be the chief characteristic. In neurasthenics the field of vision was limited, but the perimetric line was regular and the limitation concentric for colors.

From his observations on five hypnotic subjects, Ottolengni concludes: 1, that in case of especially hysterical hypnotic subjects the field of vision can be normally extended; 2, in a state of monoideism, of psychic exaltation, the limits of the field of vision are much extended, but do not go over the physiological limits; 3, in the hypnotic state the field of vision does not vary notably. The variations depend upon the state of psychic excitation in which the subject finds himself. When the subject is in a tranquil somnambulistic state, the field of vision varies least in extension but it can become irregular if the subject does not readily perceive or is fatigued. The field of vision is modified by the suggestion which is exerting its influence on the subject. The greater the sensitiveness and excitability of the subject, the more regular is the periphery of the field of vision.

In the cases of four occasional criminals (women) only one case of limitation of the visual field was met with and no irregularity of the periphery. In the cases of four male occasional criminals, only two had the field of vision slightly limited. Amongst ten typical female criminals, however, all but two showed more or less limitation of the field of vision. Of eleven typical prostitutes, eight had limitation of the field of vision, eight irregular, and four broken perimetric lines. In the cases of four hysterico-epileptics the field of vision was limited, but the perimetric line was always regular; in one case lateral hemiopia was met with. Of 13 young criminals (boys), the field of vision was limited in twelve, and in six the perimetric line was broken. In five cases the limitation assumed the form of partial vertical hemiopia. Amongst eight epileptic boys the field of vision was considerably limited for both the eyes in five cases, in four cases the periphery was irregular, and in three cases partial vertical hemiopia was found. Ottolenghi considers that these results cast doubt on the opinion of Schule that in epileptic children the field of vision is not injured. He considers that these new observations confirm the fact ascertained before that the extension and the regularity of the field of vision follow very faithfully the variation of the psychic state of the individual.

A. F. CHAMBERLAIN.

BOSTWICK, *Estimates of distance*, Science 1892 XIX 118.

The reproduction of small distances is not influenced by immediately preceding reproductions. The mean variation in estimates depends on previous training.

E. W. SCRIPTURE.

A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

By Edmund C. Sanford, Ph. D.

(*Third Paper.*)

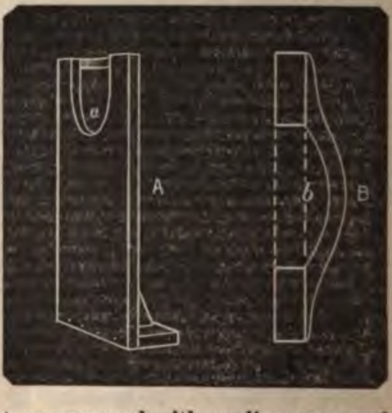
V.—VISION.

The Mechanism of the Eye, and Vision in General.

Apparatus. Many of the experiments of this section can be performed with very simple apparatus, made on the spot. The following materials will be needed: Pins, cards, corks, a candle, a couple of postage-stamps, a watch glass, pieces of colored glass, black and white card-board (not shiny), colored papers, a light wooden rod. Four inches square is a convenient size for the glass, of which two pieces should be cobalt blue, one red. Any colored papers will serve; those made for artificial flowers are easy to get in large variety of tints. A fine series of papers in Helmholtzian colors is sold by R. Jung, Heidelberg. In addition to these supplies there is need of a double convex lens of short focus, two inches or more in diameter; an ordinary burning or reading glass would do, though those mounted on an adjustable stand, costing $2.50 and upward from the physical instrument dealers, are more convenient; also a concave spectacle lens.

For Ex. 99 a pink-eyed rabbit and a little modeling clay are necessary.

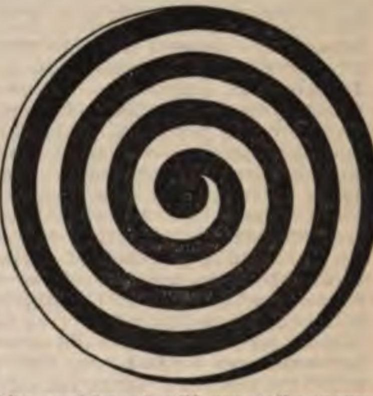
An instrument for facilating Ex. 103 (a Phakoscope) can be had from Jung for 25 marks; a more elaborate instrument of the same name is quoted by the Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, England, for £ 8-8.



For Ex. 109 and other experiments a firm head rest of some sort is required. For most purposes one like that shown in the cut will answer well enough and can easily be made. Fig. A shows a board about 20 in. high and 12 in. wide with a U-shaped opening cut in the top to receive the face, the chin resting at *a*. Fig. B, the top view, shows the cross piece against which the forehead rests at *b*. The whole when in use is clamped to the edge of the table. When a complete immobility of the head is desired it is best secured by providing a thin board cut out so that it can be put into the mouth and taken between the jaws. If the parts upon which the teeth rest are covered with sealing wax and are bitten upon while the wax is still soft, not only is a firm support for the head secured, but the head can be returned again exactly to its former position after an interval, if desired. Such a mouth board could easily be added to the support shown in the cut. For pictures of such mouth boards cf. Hermann, Handbuch der Physiol. III, pt. 2, pp. 440, 473 and 478, also Helmholtz, Optique physiologique, p. 665 (p. 517 of the first edition), Aubert, Physiologische Optik, p. 647.

For Ex. 110 make a saturated solution of chrome-alum in water, filter and put into a flat-sided clear glass bottle. Dilute, if necessary, till the yellow spot can be observed as described in the experiment.

Ex. 115 requires a pair of electrodes and a battery. The electrodes can be made by soldering connecting wires to plates of brass or zinc, two and a half inches wide by three long, and covering them with cloth. Some kind of a key for opening and closing the circuit and a commutator for changing the direction of the current are helpful, though not essential. Any battery giving a sufficiently strong current will do; one of four cells of the "gonda" pattern has proved sufficient for demonstrative purposes, and a very much weaker one will serve for showing the flash from electrical stimulation.



Ex. 119, involves a rotation apparatus of some kind and a disk traced with a spiral as in the cut. Any rotation apparatus will do, but if the laboratory is supplied with batteries one of the small electric motors now to be had at a very low price is easily adaptable for use and is extremely convenient.[1] A Porter motor, retailing at $3.00, has been used with success in this laboratory. It is well to have the disk large, a foot in diameter, and the line of the spiral thick, three eighths of an inch across, and a good black.

In a number of experiments black or white screens are to be used. A simple piece of black or white card-board will generally answer but sometimes the more permanent form indicated in the cut is convenient. It consists of an upright board 18 inches high, seven eighths of an inch thick and 12 inches wide, firmly fixed on a wooden base. In the cut the base is made too large. One side of the upright is covered with black card-board (or painted a dull black), the other with white card-board.

Of models helpful in understanding the mechanism and functions of the eye there are a number. Anatomical models are quoted, among others, by Jung (12 marks); by Kny & Co., 17 Park Place, New York, ($5.00 to $28.00); by Queen & Co., 924 Chestnut St., Philadelphia (Auzoux models, $19.00 and $20.00 without duty). Of physiological models, the best for accommodation and the like is Kühne's optical eye made by Jung, at 65 marks, by the Cambridge Scientific Instrument Co., at £7. The action of the muscles and the behavior of the eye in motion is illustrated by the Ophthalmotrope, described with cut by Helmholtz, Optique physiologique p. 678, (p. 527 in the German edition). This instrument is to be had of Jung, at 25 marks. of the Cambridge Scientific Instrument Co., for £10; and of other dealers also. Another instrument for the same purpose, called the Blemmatotrope is described by Hermann in Pflüger's Archiv, VIII, 1873, p. 305. The motions of the eye and their effect on the retinal image, such especially as those mentioned in Ex. 123, are finely shown by the Phenophthalmotrope of Donders, described in v. Graefe's Archiv für Ophthalmologie, Bd. XVI, 1870, and sold by Jung at 30 marks. An improved form of the instrument is to be had of D. B. Kagenaar, Rijks-Universiteit, Utrecht, at 40 guilders. Suggestions for simple illustrative apparatus will be found with the description of the experiments.

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[1] For fuller information on rotation apparatus see the introduction to the section on color-vision, to follow.

Standards and rods with clamps and universal joints, thought not distinctively for visual experiments, are by far the most important of the general conveniences of a laboratory. They enter into the setting up of very many experiments and a liberal share of even a small appropriation may well be invested in them. Ordinary clamps can be bought in all sizes at the hardware stores at prices from ten cents upward. The standards and couplers to be had from the chemical and physical instrument dealers are made for another purpose and are not very satisfactory in the psychological laboratory. Those made for physiologists and photographers are better. Wilhelm Petzoldt, Bairische Str. 13, Leipsig, makes a considerable variety, of which the following have been found useful in the physiological and psychological laboratories of Clark University. Standards: simple tripods with interchangeable rods of 9 and 13 mm. diameter, 6.50 marks, and large tripods with leveling screws in two of the feet and carrying two of the above mentioned rods, at the same time, 16 marks. Taple-clamps, which screw on to the edge of the table and are bored to receive the rods, thus taking the place of tripods: two kinds, one bored for the 9 mm. rods, but having only a vertical hole, 2.75 marks; the other bored for 13 mm. rods having both horizontal and vertical holes, 3.50 marks. Couplers to fit both sizes of rods: those for the 13 mm. rod (of iron) and connecting the rods only at right angles, 2 marks, those for the 9 mm. rods (of brass) and connecting the rods either at right angles or parallel 2.75 marks. Petzoldt also makes small clamps of various sizes, like those furnished with the chemical sets, mounted upon the 9 mm. rods, at 3 marks. The advantage of these rods and couplers is that they fit nicely and can be set up so as not to wobble. By using several rods and couplers a universal motion can be secured, but not so conveniently, as by the ball-joint clamps and swivel couplers made for photographers' use by Otis C. White, of Worcester, Mass. These allow extreme freedom of movement, and when fastened do not slip nor wobble. The ball joints are made to clamp on the edge of the table or to screw upon the end of rods. The first can be had in great variety of sizes, a convenient one fitting half inch rods costing $1.25. The swivel couplers allow the coupling of the rods in any position relative to each other, those of size to connect half inch and quarter inch rods costing 50 cents. Rods of various diameter and length may also be had with the ball-joints and swivel clamps. In purchasing for a laboratory from several makers it would be well to fix upon standard sizes for rods and fittings so that all may be interchangeable; and also to fix upon a standard size and number of threads to the inch for all screws cut upon the rods so that any clamps, pulleys or other small pieces of apparatus, made to screw upon one, will fit all.

On Vision in general cf. Helmholtz, Handbuch der physiologischen Optik. (The second German edition has reached page 400 : the latest complete edition is the French translation, Optique Physiologique, Paris, 1867). Aubert, Grundzüge der physiologischen Optik, Leipzig, 1876, (a portion of Graefe and Saemisch's Handbuch der ges. Augenheilkunde). Le Conte, Sight, New York, 1881. Beaunis, Nouveaux Éléments de Physiologie Humaine, Paris, 1888, (Beaunis, like Helmholtz, gives bibliographies). Wundt, Physiologische Psychologie, II, 82–209. Hermann's Handbuch der Physiologie, Bd. III, Th. 1, Leipzig, 1879.

The references following the experiments below are made chiefly to Helmholtz, the pages of the new German edition, the French edition, and, in parenthesis following the latter, of the first German edition being given, but the experiments of this section are more or less fully discussed in almost all of the works just mentioned and in many others besides.

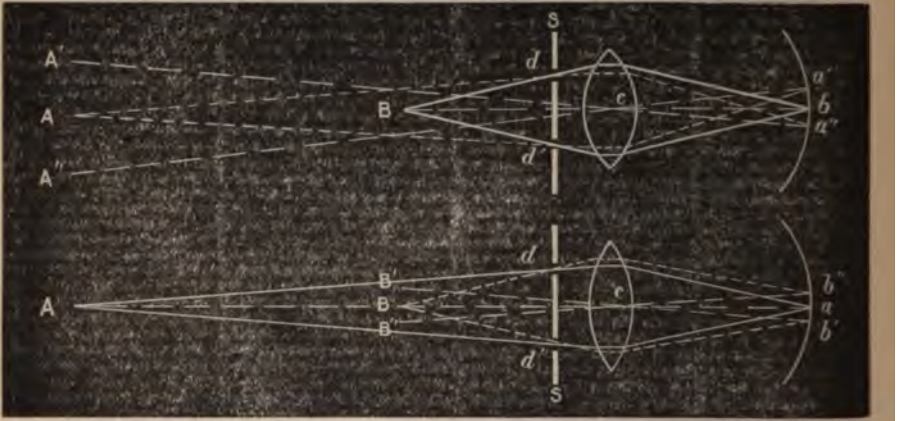
99. The retinal image. The mechanisms of the eye accomplish two things: the projection of a well defined image on the retina; and the ready shifting of the eye so as to bring successive portions of the image into the best position for vision. The retinal image is readily seen in the unpigmented eye of a pink-eyed rabbit. Chloroform the rabbit, remove the eyes and mount them in clay for readier handling. Make a thick ring of clay with an internal diameter a little greater than that of the cornea of the rabbit's eye, place the eye cornea downward in the ring and lay a similar ring upon it to keep it in place. It can now be handled easily and turned in any direction. Turn it toward the window and from behind observe the inverted image on the retina. Bring the hand into range and move it to and fro; observe that the image of distant objects is more distinct than that of the hand. If convex and concave lenses are at hand (spectacle lenses will answer) bring them before the eye and observe that the effect upon the retinal image is similar to that seen subjectively when they are held before the observer's own eye. Reverse the eye, holding it retina side toward the window, and observe the radiating and circular fibres of the iris. The eye must be fresh, for if long removed it loses its transparency.

100. Accommodation. The sharpness of the retinal image depends on the adjustment of the crystaline lens, which must be such as to focus the light from the object under regard upon the retina. The lens must be thicker and rounder for near objects, thinner and flatter for more distant ones. These adaptations of the eye are known as *Accommodation*. The changes in the clearness of the retinal image are easy to observe subjectively. Hold up a pin or other small object six or eight inches away from the eyes. Close one eye and look at the pin with the other. The outline of the pin is sharp, but the outlines of things on the other side of the room behind it are blurred. Look at these and the outline of the pin becomes blurred. Notice the feeling of greater strain when looking at the nearer object. The experiment is somewhat more striking when the nearer object is a piece of veiling or wire gauze and the farther a printed page.

On this and the next two experiments cf. Helmholtz, Physiologische Optik, 2nd Ed. pp. 112–118, French ed. pp. 119 (90)–126 (96).

101. Accommodation. Scheiner's experiment. *a*. Pierce a card with two fine holes separated by a less distance than the diameter of the pupil, say a sixteenth of an inch. Set up two pins in corks distant respectively eight and twenty inches in the line of sight; close one eye and holding the card close before the other with the holes in the same horizontal line look at the nearer pin; the farther pin will appear double; look again at the nearer pin and while looking cover one of the holes with another card; one of the images of the farther pin will disappear, the left when the left hole is covered, and the right when the right is covered. Look at the further pin or beyond it and repeat the covering, covering the left hole now destroys the right image of the nearer pin, and covering the right destroys the left. Why this should be so will be clear from the diagrams below. The upper diagram illustrates

the course of the rays of light when the eye is accommodated for the nearer pin; the lower diagram when it is accommodated for the farther pin. *A* and *B* represent the pins; *S* and *S* the pierced screen; *d* and *d'* the holes in the screen; *c* and *c* the lens; *a'ba''* and *b''ab'* the retina; *A'*, *A''*, *B'*, and *B''*, the positions of the double images; the solid lines the course of the rays from the pin accommodated for; the dotted lines the course of the rays from the other pin; the lines of dashes the *lines of direction*, *i. e.*, those giving the direction in which the images appear to the observer. In the upper diagram the rays from *B* are focused to a single retinal image at *b*, while those from *A*, being less divergent at first, are brought to a focus nearer the lens, cross over and meet the retina at *a'* and *a''*, and since each hole in the screen suffices to produce a retinal image, cause the pin to appear double, and its two images are referred outward as usual with retinal images along the lines of direction, (which cross a little forward of the back surface of the lens, in the *crossing point of the lines of direction*), the right retinal image corresponding with the left of the double images and *vice versa*. If now the right hole in the screen be closed the left retinal image and the right double image disappear. The case of accommodation for the farther pin will be clear from the lower diagram, if attention is given to the dotted and dashed lines. It will also be easy to explain why moving the card when looking through a single pin hole causes apparent movements of the pin not accommodated for, and why in one case the movement seems to be with the card and in the other case against it. *b*. Stick the pins into the corks so that they shall extend horizontally, and examine them with the card so held as to bring the holes above one another. *c*. Arrange the holes thus: . • . and observe that the triple image of the nearer pin (when the farther is fixated) has the reverse figure • . • Scheiner's experiment can easily be illustrated with a double convex lens and a pierced screen of suitable size.

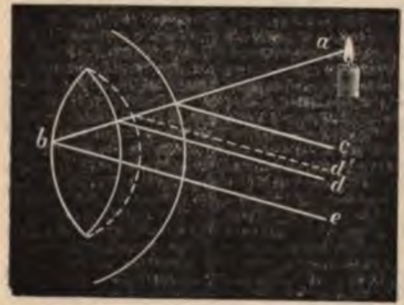


102. The Range of accommodation. *a*. Find by trial the nearest point at which a pin seen, as in Ex. 101, can be seen single. This is the *near point* of accommodation. For the short-sighted a *far point* may also be found, beyond which double images reappear. *b*. Find how far apart in the line of sight two pins may be and yet both be seen single at one and the same time. Try with the nearer at 20 cm., at 50 cm., at 2 m. That portion of the line of sight, for points in which the same degree of

accommodation is sufficient, is called the *line of accommodation*. The length of the line increases rapidly as the distance of the nearer object from the eye increases.

Cf. Helmholtz, *op. cit.* G. 114, 119, Fr. 122 (93), 123 (97).

103. The Mechanism of accommodation. *a.* The change in the lens in accommodation is chiefly a bulging forward of its anterior surface. This may be observed as follows. Let the subject choose a far and a near point of fixation in exactly the same line of vision, close one eye and fix the other upon the far point. Let the observer place himself so that he sees the eye of the subject in profile with about half the pupil showing. Let the subject change his fixation at request, from the far to the near point, being careful to avoid any sidewise motion of the eye. The observer will then notice that more of the pupil shows and that the farther side of the iris seems narrower. This change is due to the bulging forward of the front of the lens. If the change were due to accidental turning of the eye toward the observer the farther edge of the iris should appear wider instead of narrower. *b.* Purkinje-Sanson images. The changes in the curvature of the lens may also be observed by means of the images reflected from its front or back surfaces and from the front of the cornea. Operate in a darkened room or at night. Let the subject choose far and near fixtation points as before. Let the observer bring a candle near the eye of the subject at a level with it and a little to one side and place his own eye in a position symmetrical to the candle on the other side of the subject's line of sight. Careful examination will show three reflected images of the flame; one on the side of the pupil next the light, easily recognizable, bright and erect, reflected from the surface of the cornea; a second nearer the centre of the pupil and apparently the farthest back of the three, erect like the first, but very indistinct, (more like a light cloud than an image), reflected from the anterior surface of the lens; and a third, a mere point of light, near the side of the pupil farthest from the flame, inverted and reflected from the posterior surface of the lens. When the observer has found these three images the subject should fixate alternately the near and far points chosen. As he fixates the near point the middle image will grow smaller, advance and draw toward the corneal image; when he fixates the far point the image will enlarge, recede and move away from the corneal image. The following diagram after Aubert illustrates the movement of the middle image; the full lines indicate the positions of the cornea and lens and the course of the rays of light when the eye is accommodated for the far point; the dotted lines indicate the anterior surface of the lens and the direction of the ray reflected from its surface when the eye is accommodated for the near point. Three images similar to those in question can be observed on a watch glass and a double convex lens held in the relation of the cornea and crystaline.



Cf. Helmholtz, *op. cit.* G. 131-141, especially 131-134, Fr. 142 (104)—154 (112), especially 142 (104)—146 (107). Aubert, Physiologische Optik, § 44.

104. Chromatic aberration. Of the various defects of the eye as an optical instrument only one will be mentioned here, namely, chromatic aberration, and that because it has been supposed to offer a possible means of inferring the relative distance of objects from the eye. The different colored rays of light are not equally refracted by the lens, the violet most, the red least, and the other colors in order between. The point at which parallel violet rays are brought to a focus is therefore nearer the lens than the point for red; and in order that the same degree of accommodation may serve to show a red lighted object and a violet lighted object at the same time and both with full distinctness, the red must be somewhat farther away. *a.* The aberration can easily be observed by looking at a small gas or candle flame through a piece of cobalt blue glass which transmits light from the two ends of the spectrum chiefly. Hold the glass eight or ten inches before the face and fixate some point on it; the flame will appear pinkish with a blue border. Fixate some point considerably beyond the flame; the flame is now bluish and the border is a fine red line. *b.* Look at the edge of the window frame next the pane, and bring a card before the eye so that about half the pupil is covered; if the card has been brought up from the frame side, the frame will be bordered with yellow; if from the pane side, with blue. In ordinary vision these fringes do not appear, because the colors overlap one another and produce a practically colorless mixture. *c.* v. Bezold's experiment. Something similar may be observed, on regarding the parallel lines of the left figure under Ex. 111 with imperfect accommodation.

Cf. Helmholtz, *op. cit.* G. 156-164; Fr. 172 (125)—179 (131). Beaunis, Nouveaux éléments de physiologie humaine, II, 506. v. Bezold, v. Graefe's Archiv f. Ophthalm., XIV, Heft 2, 1-29.

105. Accompanyments of accommodation. *a.* Notice that as the subject in Ex. 103 accomodates for a near point, his pupil grows smaller, and as he accommodates for a far point, grows larger. Cf. also Ex. 106, *b.* Degrees of accommodation suitable for objects at different distances are habitually associated with the amounts of convergence of the lines of sight necessary to fix the eyes upon such objects, and a little practice is necessary before the convergence and accommodation can be dissociated. Place a couple of postage stamps six inches apart on the table and look at them from a distance of twelve or fifteen inches with crossed eyes so that the left eye looks at the right stamp and the right eye at the left stamp; the lines of sight now cross only a few inches from the eyes and the accommodation is for that distance and not for the true distance of the stamps, as is betrayed by the blurring of their images. Holding a pencil at the crossing point of the lines of sight is helpful in first attempts at crossed vision.

Cf. Helmholtz, *op. cit.* G. 130, Fr. 142 (104).

106. Entoptic phenomena: ***Muscae volitantes***, etc. Fix a lens of short focus at some distance from a bright gas or candle flame. *a.* Set up in the focus of the lens a card pierced with a very fine hole, bring the eye close to the hole and look toward the light; the eye should be far enough from the hole to prevent the edge of the lens from being seen; the rays of light that now reach the eye are divergent and the crystaline lens does not bring them to a focus on the retina, but only refracts them to such a degree that they traverse the eye nearly parallel and thus in suitable condition for casting sharp shadows upon the retina of objects on or in the eye. The lens will appear full of light, and in it will be seen a variety of shadings, blotches and specks, single or in strings, the outward projection of the shadows just mentioned. The figures in this luminous field will vary from person to person, even from eye to eye, but in almost every eye some will be found that move and some that remain fixed and only move with the eye. Of the moving figures some are due to particles and viscous fluids on the surface of the eye; they seem to move downward and are changed by winking. Notice for example the horizontal bands that follow a slow dropping and raising of the upper

lid. Others, the *muscae volitantes* are frequently noticed without any apparatus; they appear as bright irregular threads, strings of beads, or groups of points, orsingle minute circles with light centres. They seem to move downward in the field and consequently actually move upward in the vitrious humor where they are found. Of the permanent ones, some are due to irregularities of structure or small bodies in the lens and its capsule (spots with dark or bright centres, bright irregular lines, or dark radiating lines corresponding probably to the radial structure of the lens); others of a relatively permanent character can be produced on the cornea by continued rubbing or pressure on the eyeball. *b*. The round spot of light in which these things are seen represents the pupil, and the dark ground around it the shadow of the iris. Notice the change in the size of the spot of light, as the eye is accommodated for different distances (cf. Ex. 105), and as the other eye is exposed to, or covered from, the light. The change begins in about half a second. It shows the close connection of the iris mechanisms of the two eyes and is typical of the way in which the two eyes co-operate as parts of a single visual machine. Some of these entoptic observations may be made with a pierced card alone, or simply by looking directly at a broad expanse of clear sky with out any apparatus at all.

Cf. Helmholtz, *op. cit.* G. 184-192 and Tafel I. which represents the appearance of several of the entoptic objects; Fr. 204 (149)—214 (156) and Pl. V; also pp. 548 (419)—558 (427).

107. Retinal blood-vessels, Purkinje's vessel figures. *a*, Concentrate a strong light, (prefarably in a dark room) or even direct sunlight, with a double convex lens of short focus on the sclerotic in the outer corner of the eye of the subject, requesting him to turn the eye toward the nose and giving him a dark background to look toward. Make the spot of light on the sclerotic as small and sharp as possible and give to the lens a gentle to and fro or circular motion, and after a little the subject cannot fail to see upon the field which the light makes reddish yellow the dark branching figure of the shadows of the retinal vessels. Notice that the area directly fixated, is partially surrounded, but not crossed by the vessels. In this lies the yellow spot (*macula lutea*) or area of clearest vision of the retina, not, however, to be observed in this experiment. The centre from which the vessels radiate lies in the point of entrance of the optic nerve. In this form of the experiment the light radiates in all directions within the eye from the illuminated point of the sclerotic. *b*. Somewhat the same kind of an image of the vessels is to be secured by moving a candle about near the eye, below it and a little to one side. In this experiment some indication of the region of the yellow spot is to be seen. In this form of the experiment the light enters by the pupil, forms an image on a part of the retina somewhat remote from the centre and this retinal image is the source of light by which the vessel shadows are cast. *c*. Look through a pin hole in a card directly at the clear sky or any other strongly illuminated even surface or at a broad gas flame. Give the card a rather rapid circular motion and the finer retinal vessels in the region of the yellow spot will readily be seen, among them also a small colored or slightly tinted spot (best seen perhaps by gas light) representing the *macula*, and in its centre a shadowy dot (representing the *fovea* or point of clearest vision) which appears to rotate when the motion of the card is circular. If the card is moved horizontally the vertical vessels alone appear; if vertically, the horizontal vessels. Notice also the granular appearance of the *macula*; the granulations have been supposed to represent the visual cones of that region. The finer retinal vessels can also be seen when looking at the vacant field of a compound microscope, if the eye is moved about rapidly. In all of these cases it is important that the shadows be kept moving; if they stand still, they are lost. The expla-

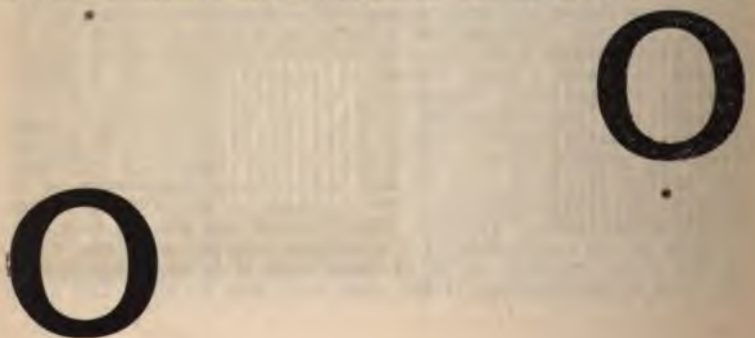
nation is partly physiological, the portions of the retina on which the shadows rest soon gain in sensitiveness enough to compensate for the less light received, and partly psychological, moving objects in general being more readily attended to, and those whose images rest continously on the retina without motion being particularly subject to neglect. Once having become acquainted with the appearance of these vessel figures it is often possible to see traces of them without any apparatus. Parts of them, with something of the projection of the yellow spot, may sometimes be seen for an instant as dark figures on the diffusely lighted walls and ceiling or as light figures on the dark field of the closed eyes when the eyes are opened and closed after a glance at the window on first waking in the morning, or in blue when looking at the snow and winking on a bright morning, or projected on the sky and keeping time with the pulse after a rapid walk up hill.

Helmholtz, *op. cit.* G. 192-198. Fr. 214 (156)—221 (161).

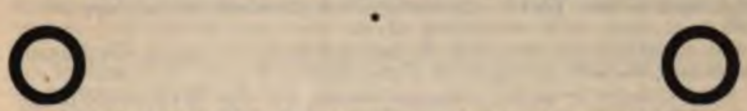
108. Retinal circulation. Look steadily through two or three thicknesses of blue glass at the clear sky or a bright cloud, and observe a large number of what seem to be bright points darting hither and thither like bees in a swarm or rapidly blown snow-flakes. Careful observation will also establish that the bright points are followed by darker shadowy ones. Pick out a speck on the window to serve as a fixation point, look at it steadily and observe that while the movements of the points seem irregular the same lines are retraced by them from time to time. When several of their courses have been accurately observed, repeat the experiment for demonstrating the finer retinal vessels (Ex. 107 *c.*) and notice that fine vessels are found which correspond to the courses which the points seem to follow. These flying points can be seen without the glass by a steady gaze at an evenly lighted bright surface, and some times a rhythmic acceleration of their movement will be found, corresponding to the pulse. Helmholtz explains the phenomenon as due to the temporary clogging of fine capillary vessels by large blood corpuscles. The bright lines (the apparent tracks of bright points) are really the relatively empty capillary tubes ahead of the corpuscles, which, after an instant, are driven onward by others crowding behind and in turn give the shadow that apparently follows the bright points.

Cf. Helmholtz, *op. cit.* G. 198; Fr. 221 (837), 555 (425), Rood, American Journal of Science, 2d Series, XXX, 1860, 264-265, 385-386.

109. The Blind-spot. Mariotte's experiment. The point of entrance of the optic nerve is unprovided with visual end-organs and is irresponsive to light. *a.* This insensitiveness is easily demonstrated with the diagrams below. Close the left eye and keeping the right fixed on the asterisk in the upper diagram move it backward and forward till a point is found where the black oval disappears. For the blind spot of the



left eye use the second diagram. The blind spot may be demonstrated simultaneously in both eyes by the use of a figure like that below enlarged a couple of times. The experimenter should look at the asterisk while he holds a sheet of paper in the median plane of his head, to prevent each eye from seeing the other's part of the diagram. *b.* To draw the projection of the blind-spot, arrange the head support described above, place opposite the face, at a distance of about 18 inches, a vertical sheet of white paper, and put a dot on it for a fixation point. Fasten upon the end of a light rod a bit of black paper about 2 mm. square or blacken the end of the rod with ink. Bring the face into position, close one eye, and fix the other upon the dot. Move the rod slowly so as to bring the little square over the part of the white paper corresponding to the blind spot, dotting on the paper the points where the square disappears or reappears. Repeat at various points till the outline of the projection of the blind spot is complete. If the mapping is carefully carried out, the map will probably show the points of departure of the large blood vessel, that enter with the nerve.

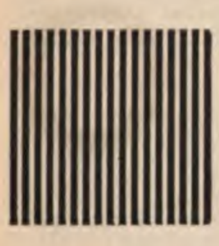


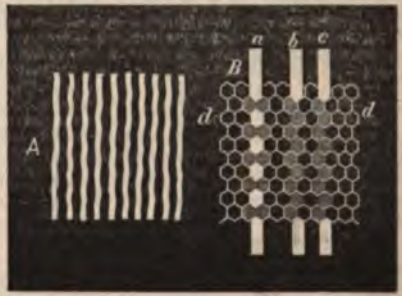
Helmholtz, *op. cit.* G. 250-254, Fr. 284 (210)—289 (214).

110. The yellow spot, *macula lutea*. The projection of the yellow spot in the visual field can be made visible in several ways. Two have already been mentioned in Ex. 107; others are as follows. *a.* Close the eyes for a few seconds and then look with one of them through a flat sided bottle of chrome alum solution at a brightly lighted surface (not yellow) or the clear sky. In the blue green solution a rose colored spot will be seen which corresponds to the yellow spot. The light that comes through the chrome alum solution is chiefly a mixture of red and green and blue. The pigment of the yellow spot absorbs a portion of the blue and green and transmits the rest, which makes a rose colored mixture, to the visual organs behind it. *b.* The region of the yellow spot may be seen as an area of somewhat deeper shade when the eye looks at an evenly lighted surface like the ceiling, and the illumination is made intermittent by moving the spread fingers to and fro between the eye and the ceiling.

Cf. Helmholtz, *op. cit.* Fr. 548 (419)—551 (421). On *a.* cf. Maxwell, On Color-vision at different points of the Retina, Report of the British Assoc., 1870; or Vol. II, pp. 230-232 of Maxwell's Scientific Papers. Cambridge, 1890.

111. Visual cones in the *fovea*. Bergmann's experiment. Place the left hand diagram in a good light and look at it from a distance of a yard and a half or two yards. Observe the apparent bending and beading of the lines. This is supposed to be due to the mosaic arrangement





of the visual cones. The cones that are touched by the image of one of the white lines are stimulated in proportion as they are more or less touched. Those that are much stimulated furnish the sensation of the white line and its irregularities, those that are little stimulated join with those that are not touched at all to give the image of the black line and its irregularities. This is schematically represented in the right hand cut.

Cf. Helmholtz, *op. cit.*, G. 257-258, Fr. 293 (217)—294 (218). Bergmann, Zeitschrift für rat. Med.. (3), II, 88.

112 Acuteness of vision, *minimum visibile*, and size of the cones in the *fovea*. Place the parallel line diagram used in the last experiment in a good light and walk backward from it till the lines can just no longer be distinguished as separate. If the experimenter's eyes are not normal he should use glasses that fit his eyes for distinct vision at the distance required. Measure the distance between the eye and the diagram and calculate the angle whose apex lies in the crossing point of the lines of direction (about 7 mm. back of the cornea and 16 mm. in front of the retina) and whose base is the distance from the middle of one line of the diagram to the middle of the next; in this diagram 1.58 mm. This angle measures the least visible extent when discrimination is involved; the least luminous extent that can still impress the retina is far smaller, as witness the visibility of the stars. On the supposition that if the sensations of two cones are to be separable they must be separated by an unstimulated, or at least by a less stimulated, cone, it has generally been considered that the cones could not subtend a greater angle than that found in this experiment, 60″—90″, representing 0.004—0.006 mm. on the retina, and this agrees well with microscopical measurements. But as Helmholtz notices (Phys. Opt. 2nd ed. p. 260) this experiment does no more than prove that there are on the retina rows of sensitive elements the middle lines of which are separated by the angular distance found in the experiment. The elements themselves, if properly arranged may be somewhat larger. Calculation of the number of such elements in a sq. mm. of the retina, based on this view of the experiment agrees well in the case of Helmholtz's own determination with the result of microscopical counting. *b*. The discriminative power of the retina falls off rapidly in all directions from the *fovea*, more rapidly above and below than in a horizontal direction. Arrange a head rest and perpendicular plane as in Ex. 109 *b*. Place upon the end of the rod used in that experiment a card on which have been made two black dots 2 mm. in diameter and 4 mm. from centre to centre. Move the card horizontally toward the fixation point, beginning beyond the point at which the two dots can be distinguished and moving inward till they can just be distinguished. Measure the distance from the fixation point and repeat several times both to the right and left of the fixation point and above and below, holding the card so that both dots are in each case equally distant from the fixation point.

Helmholtz, *op. cit.*, G. 255—264, Fr. 291 (215)—301 (223).

113. Mechanical stimulation of the retina. *a*. Phosphenes. Turn the open or closed eye as far as possible toward the nose and press on the eye-lid at the outer corner with the finger or the tip of a pen holder. On the opposite side of the visual field will be seen a more or less complete circle of light surrounded by a narrow dark band, outside of which again is a narrow band of light. Notice the color of the light seen. Get the phosphenes by pressure at other points of the eye ball. *b*. Press the eye moderately with some large object, say the angle of the wrist when the hand is bent backward, and continue the pressure for a minute or two. Peculiar palpitating figures will be observed and

strange color effects. The former Helmholtz compares to the tingling of a member that is "asleep." *c.* Standing before a window, close the eyes and turn them sharply from side to side. As they reach the extreme position in either direction observe immediately in front of the face a sudden blue spot surrounded by a yellow band. A second fainter spot farther from the centre in the direction of motion may also be seen. The yellow ring is due to the stimulation of the portion of the retina in the region of the blind spot in the eye that turns inward. The blue spot represent the blind spot in the same eye. Cf. explanation in the latter part of Ex. 115.

Helmholtz, *op. cit.* G. 235-239, Fr. 266 (196)—270 (200). Le Conte, American Journal of Psychology, III, 1889-90, 364—366.

114. Idio-retinal light, light chaos, light dust. Close and cover the eyes so as to exclude all light, or experiment in a perfectly dark room. Let the after effects of objective light fade away and then watch the shifting light clouds of retinal light. The cause of the retinal light is not altogether clear, but it is supposed to be a chemical action of the blood on the nervous portion of the visual apparatus. Aubert estimates its brightness at about half the brightness of a sheet of paper illuminated by the planet Venus when at its brightest. *b.* When awake in the night time in a room that is almost perfectly dark (e. g. in which the form of the window and the large pieces of furniture cannot be made out) notice that the white clothing of the arms can be seen faintly as they are moved about, but not when they are still. In the last case the very faint light they reflect is not sufficient to make them distinguishable from clouds of idio-retinal light.

Cf. Helmholtz: *op. cit.* G. 242-243, Fr. 274 (202)—275 (203). On *b.* cf. Helmholtz, Die Störung der Wahrnehmung kleinster Helligkeitsunterschiede, Zeitschrift für Psychologie, I, 1890, 6-9.

115. Electrical stimulation of the visual apparatus. Moisten throughly with strong salt water both the electrodes and the portions of the skin to which they are to be applied. Place one of the electrodes on the forehead (or on the edge of the table and lay the forehead upon it), the other on the back of the neck; or, if the current is strong enough, hold it in the hand or lay it on the table and put the hand upon it. At each opening or closing of the circuit a bright flash will be seen, whether the eyes are closed or open. With the eyes closed and covered the effects of the continuous current may be observed. In this case it is well to apply the electrode slowly and carefully so as to avoid as much as possible the flash caused by the sudden closing of the circuit. When the positive electrode is on the forehead, the negative on the back of the neck a transient pale violet light will be seen distributed generally over the field and forming a small bright spot at its centre. Sometimes traces of the blind spot appear. The violet light soon fades and on opening the circuit, there is a notable darkening of the field with a momentary view of the blind spots as bright disks. When the negative electrode is on the forehead, the positive on the back of the neck, the phenomena are in general reversed, the darkening occuring on closing the circuit, the violet light on opening it. Helmholtz sums up these and other experiments as follows: "Constant electrical circulation through the retina from the cones toward the ganglion cells gives the sensation of darkness, circulation in the contrary direction gives the sensation of brightness." (Phys. Opt. 2nd ed. 247). That the blind spot should appear as a disk of different color from the rest of the field seems to be due to the fact that the sensitive parts of the retina immediately surrounding it are somewhat shielded from the electric current, and as usual their condition is attributed to the blind spot also. The experiment is not entirely a pleasant one, on account of the feeling

which the current produces in the head, the electrical taste in the mouth and the reddening of the skin under the electrodes.

Cf. Helmholtz, *op. cit.* G. 243-248, Fr. 275 (203)—281 (207).

116. After-images, accidental or consecutive images. After-images in which the relations of light and shade of the original object are preserved are called *Positive After-images.* Those in which these relations are reversed (as in a photographic negative) are called *Negative After-images. Positive after-images* are of changing colors, but most important to notice here are those of the color of the object (*like colored*), and of the complementary color (*opposite colored*). *Negative after-images*, so far as observed, are always *opposite colored.* All after-images, especially the positive, can best be observed in the morning when the eyes are well rested. *a.* Negative after-images: look steadily for a minute at a fixed point of the window, then at a white screen or an evenly lighted unfigured wall; the dark parts of the window will now appear light and *vice versa.* Get a lasting after-image and look at a corner of the room or at a chair, or other object of uneven surface; notice how the image seems to fit itself to the surface upon which it rests. After a little practice it is also possible at desire to see the image floating in the air instead of lying on the back-ground. *b.* Look steadily at a bright colored object or some bits of colored paper, then at the screen; observe that the colors of the after-images are approximately complementary to the colors of the objects producing them. Negative after-images are some times very lasting and for that reason are those most frequently noticed in ordinary experience; they are a phenomenon of retinal fatigue. *c.* Positive after-images. Look for an instant (one-third of a second) at the window, then close and cover the eyes, or look at a dark surface; for a very short time an after-image like the original object in color and distribution of light and shade can be seen. The positive after-image is of short duration and is not so readily observed as the negative; it is a phenomenon of retinal inertia, of the prolongation of retinal excitation. *d.* Colored positive after-images. Look for an instant at a gas flame through a piece of red glass, then close the eyes and observe the red image; repeat the experiment continuing the fixation of the flame for half a minute; the resulting after-image will be bright as before but of the complementary color. *e.* Get an after-image of the window of not too great an intensity, and alternately project it on a sheet of white paper and the dark field of the closed and covered eyes; it will be found negative on the white back-ground and positive on the dark. *f.* Get a good after-image of the window and observe with closed and covered eyes the play of colors as the image fades. Try several times and observe that the order of succession is the same.

Cf. Helmholtz, *op. cit.* Fr. 446 (338), 471 (357)—500 (380). Wundt, Physiologische Psychologie, 3rd ed I, 472-476.

117. Effect of eye-motions on after-images. Get a moderately strong after-image of the window; look at the wall and keep the eyes actively in motion; the image will be seen with difficulty while the eye is in motion; when the eye is brought to rest, however, it will soon appear. In general any visual stimulus that moves with the eye is less effective than one that does not.

Cf. Exner, Das Verschwinden der Nachbilder bei Augenbewegungen. Zeitschrift für Psychologie, I, 1890, 47-51.

118. The seat of the after-image. An after-image due to exclusive stimulation of a single eye may under proper conditions sometimes seem to be seen with the other unstimulated eye. From this it has been inferred that the seat of after-images was central, not peripheral; that is, in the visual centres of the brain, not in the

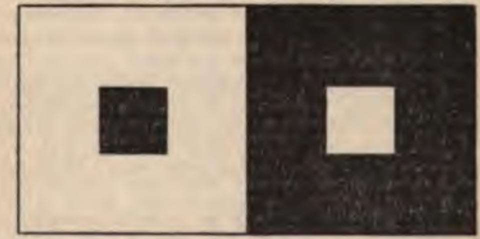
eye. The following experiments show, however, that the after-image is really seen with the eye first stimulated, and so render the hypothesis of a central location unnecessary. *a*. Look steadily for several seconds at a bit of red paper on a white ground, using only one eye, say the right, and keeping the other closed; when a strong after-image has been secured, remove the paper, close the right eye, open the left and again look steadily at the white ground; after a little the field will darken and the after-image will reappear. If the red does not produce a sufficiently lasting image, substitute for it a gas flame or some other bright object. That we have really to do, however, with the eye originally stimulated, (its present dark field being superposed upon the light one of the other eye) appears from the results of *b* and *c*. *b*. Get the after-image as before; then open both eyes and bring a bit of cardboard before the eyes alternately; bringing it before the left eye rather brightens the image; bringing it before the right dims or abolishes it; the image is therefore chiefly affected by what affects the right eye. *c*. Get the after-image again and close and cover both eyes; observe the the color of the after-image as projected on the dark field; then open the left eye, letting the right eye remain closed and covered; the after-image will be seen, not in the color it has when the right eye is open and the image is projected in the light field, but in that which it has in the dark field of the closed eye.

Cf. Delabarre, On the seat of Optical After-Images, American Journal of Psychology, II, 1888-89, 326-328.

119. After-images of motion. Fasten upon the rotation apparatus a disk like that in the first cut on page 475. Then look at a page of print or into the face of a by-stander and notice the apparent shrinking (if the spiral has seemed to run outward) or swelling (if the spiral has seemed to run inward). Illusions of increase or decrease of distance sometimes accompany those of motion. These after-images of motion have been explained as due to unconscious persisting movements of the eyes. This is probably incorrect, for in the present case it would seem necessary that the eyes should move in all directions at the same time.[1]

Cf. Helmholtz, *op. cit.* Fr. 766 (603)—769 (605). Bowditch and Hall, Optical Illusions of Motion, Journal of Physiology, III, 297-307. Mach, Bewegungsempfindungen, Leipzig, 1875, pp. 59-61, (see also pp. 61-65 for yet another kind of after-image) and Analyse der Empfindungen, Jena, 1886, pp. 65-67.

120. Irradiation. This term is used to designate the apparent enlargement of bright surfaces at the expense of adjacent dark surfaces. It is most strongly marked when the bright surface is intense and the accommodation is imperfect, but is not absent with perfect accommodation. Even with perfect accommodation, and much more so with imperfect accommodation, the line of juncture of a bright and dark surface is not really a sharp line but a narrow band of gray of which more than



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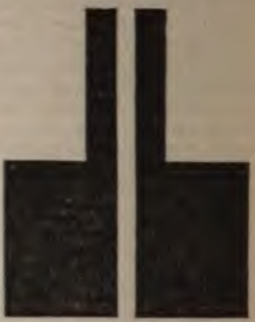
[1] My assistant Mr. T. L. Bolton, has noticed that these after-images are subject to illusory transference like those of Ex. 118.

the proper amount is credited to the white, for reasons to be brought out in the section to follow on the Psychophysic Law. The following are some of the common cases of irradiation: *a*. Hold a ruler or a straight edged piece of black card-board close before a gas or candle flame so as to cover a portion of it, and notice that the flame seems to cut into the edge, and if there are differences in brightness the brightest parts cut in deepest. *b*. Notice that the white squares in the diagram below, when brought into a strong light, seem larger than the black, though they measure the same in size.

c. Irradiation of dark lines. A black line on a white surface (or a white line on a black surface) may some times be enlarged by the greater part of its gray fringe, because near the outer edge of the fringe the blackness (or for white lines, the whiteness) decreases very rapidly and so seems to make a boundary. Look at the accompanying diagram through a lens that will make accommodation very imperfect. The narrow black strips will appear larger for the reason just mentioned, while the lower black areas will be cut into as in the ordinary cases of irradiation, giving to the white stripe between the shape of a club with the handle uppermost. Helmholtz suggests with reason that these two phenomena, having quite different causes, should have different names, and the term "irradiation" be confined strictly to such enlargement of white surfaces as takes place with exact accommodation.



Cf. Helmholtz, *op. cit.*, G. 394-400, Fr. 425 (321)—433 (327).

121. Reflex movement of the eye. The eye is a moving as well as a seeing member and its motor functions are of great importance for psychology. Of the first importance is the constant reflex tendency of the eye to move in such a way as to bring any bright image lying on a peripheral part of the retina, or any to which attention is directed, into the area of clearest vision. Many evidences of this tendency will be found in the ordinary course of vision. By way of experiment, try to study attentively a *musca volitans* or a negative after-image that is just to one side of the direct line of sight. The apparent motion of the object measures the energy of the reflex.

122. Associated movements of the eyes. The two eyes form a single visual instrument and even when one eye is closed it follows to a considerable degree the movements of its open companion. *a*. Close one eye and, resting the finger-tip lightly on the lid, feel the motions of the eye as the other looks from point to point of the visual field. *b*. Get a monocular after-image as in Ex. 118 and when it has become apparently visible to the open eye, notice that it seems to accompany that eye as it takes one fixation point after another in the field of regard.

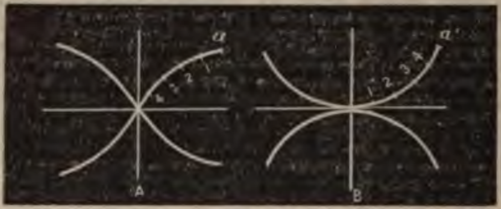
123. Motions of the eyes when the lines of sight are parallel, Donders's and Listing's laws. All motions of the eye can be interpreted as rotations of greater or less extent about one or more of three axes: a sagittal axis, corresponding nearly with the line of sight; a frontal axis, extending horizontally from right to left; and a vertical axis. All these intersect in the centre of rotation of the eye. Now it is easily conceivable that for any position of the line of sight, *e. g.* 15° to the right and 10° upward, there would be an infinite number of positions that the eye might assume by rotation about the line of sight itself. As a matter of fact, however, it does not assume an indefinite number of positions, but one and only one, no matter by what route the line of sight may have come to that point. This is the *law of constant orienta-*

tion or Donders's law. Listing's law goes further and asserts that the position is not only fixed, but is such as the eye would assume if the line of sight were moved from its primary position (approximately that in which the eye looks straight forward to the horizon) to the point in question without any rotation at all about the line of sight, but about a fixed axis standing perpendicular at the centre of rotation to both the primary and the new position of the line of sight. The advantage to vision of the constancy of orientation and the exclusion of rotation about the line of sight is considerable, especially in determining directions in the field of regard. The correctness of these laws is easy to demonstrate. *a.* Donders's law. Cut in a sheet of black cardboard two slits an eighth of an inch wide and six or eight inches long, crossing at right angles. Set the cardboard in the window or before some other brightly lighted surface. Arrange a head rest at some distance and when the head is in position, get a strong after-image of the cross, fixating its middle point. Then, without moving the head, turn the eyes to different parts of the walls and ceiling. The image will suffer various distortions from the different surfaces upon which it is projected, but each time the eye returns to the same point the image will lie as before. If the wall does not offer figures by which this can be shown, have an assistant mark the position of the image upon it. The after-image is of course fixed on the retina and can move only as the eye moves. *b.* Listing's law. Make over the cross used in *a* into an eight rayed star by cutting two other narrow slits across its centre. Arrange the card before a brightly lighted wall and parallel to it at a height a little less than that of the eyes when the head is in position. Draw lines or stretch threads on the wall that shall appear to continue the rays of the star upward and right and left, and downward if convenient. Fix the head rest directly before the star at a distance of five or six yards or more. Adjust the head so that when the after-image of the star is carried along the horizontal or vertical line its corresponding ray will coincide exactly with the line. When this condition is fulfilled for both lines the eyes and lines of sight are in the *primary position*. When the primary position has been found, carry the after-image along the lines prolonging the other rays and observe that as before the after-image of the ray coincides with its line. This would be found true, for all except extreme positions, of all other rays, and shows that the eye does not in such motions rotate about the line of sight. *c.* In motions from other or secondary positions, however, there is such a rotation. Turn the head somewhat to one side or tip it forward or backward from the primary position, repeat *b* and notice that the lines of the after-image betray some rotation.

Cf. Helmholtz, *op. cit.*, Fr. 601 (462)—610 (470), 621 (479) ff. Le Conte, Sight, pp. 164-177.

124. Actual movements of the eyes. Rapid motions of the eyes are not executed with mechanical exactness according to Listing's law, though it gives correctly the end position reached. The axis of rotation is not quite constant and the lines passed over by the point of sight are therefore not quite straight. This is easy to observe as follows. In a dark room turn down the gas till it burns in a flame not more than 8 or 10 mm. high. Then using this as a point of departure in the primary position look suddenly from it to other points of fixation in various directions about it, and notice the shape of the long positive after-images that result from the motion of the image of the flame, over the retina. These will probably have the shape of the radii in the left hand figure below. The newest part of the after-image is that next the li[illegible] the oldest part is that next the fixation point, for example a[illegible] points of the after-image curve are now interpreted in the or[illegible]

appears that the eye at first moved rather rapidly toward the right but rather slowly upward, while at last it moved rather slowly toward the right and rapidly upward. Plotting the curve accordingly we get the reverse curve shown in *B* which shows the true track of the fixation point. It is said that for some eyes the after-images, though curved, do not coincide with those figured in *A*.



Cf. Wundt. Beiträge zur Theorie der Sinneswahrnehmung, Leipsig, 1862, pp. 139 ff. 202. Hermann's Handbuch der Physiol. III, Th. 1, 450-451.

125. Convergent movements of the eyes. When the lines of sight converge, the movements of the eye do not follow Listing's law. When the lines of sight converge in the primary position both eyes rotate outward; as the lines of sight are elevated, the convergence remaining the same, the outward rotation increases; as they are depressed, the rotation diminishes and finally becomes zero. On a sheet of cardboard draw a series of equi-distant parallel vertical lines one or two inches apart and eight or ten inches long, drawing the left half of the group in black ink, the right half in red. Cross both sets midway from top to bottom by a horizontal line, red in the red set and black in the black set. Fasten the cardboard flat upon a vertical support and arrange the head rest in front of it.. The horizontal line of the diagram should be on a level with the eyes. *a*. Fasten a bit of wire vertically between the eyes and the diagram in such a way that it can be moved to and from the eyes. Bring the head into position and look at the wire, but give attention to the diagram. It will be seen that the red and black lines are not quite parallel and that they are less nearly so as the wire is brought nearer the face. The red lines (seen by the left eye) seem to incline a little toward the right and the black lines (seen by the right eye) toward the left. As the wire comes near and the convergence is great the horizontal lines will also show the rotation. This apparent rotation of the lines is not, as in the case of the after-images, a sign that the corresponding eye has rotated in the way that they have, but that it has rotated in the opposite way. *b*. Repeat this with the head much inclined forward (the equivalent of elevating the eyes) and with it thrown far back (equivalent of depressing the eyes) taking care that the wire is always at the same distance from the eyes. In the first case the apparent rotation of th lines is increased, and in the second decreased to zero or even transformed into rotation in the opposite direction.

Cf. Helmholtz. *op. cit.* Fr. 609 (469)—610 (470). Le Conte, Sight, 177—191. Hermann's Handbuch der Physiol. III, Th. 1, 496 ff.

126. Involuntary movements of the eyes. Lay a small scrap of red paper on a large piece of blue. Fixate some point on the edge of the red. After a few seconds of steady fixation, the color near the line of separation, will be seen to brighten, now in the red and now in the blue. This is due to the small unintentional movements of the eyes.

LETTERS AND NOTES.

LONDON, April 8, 1892.

TO THE EDITOR.

It been suggested to me that a short account of the various opportunities which exist in London for the study of Philosophy in its different branches, embracing Psychology, Logic and Ethics, would be acceptable to your readers. I have therefore put together a few notes showing what is done at the various Institutions in the Metropolis which make the teaching of any of the branches of Philosophy a systematic part of their work. The account does not claim to be exhaustive and may not even do full justice to some of the Institutions named, though it seeks to do this as far as published materials permit. The information is derived partly from personal knowledge and partly from the calendars and printed syllabuses, and if not very complete it may be interesting to Americans who are devoting so much attention to philosophical studies.

Naturally one begins with the University of London which may be presumed to be the chief influence in directing the line of study followed in the London Colleges. In Mental Science the influence of James Mill, and later Grote and Bain as examiners, did much to fix the schedule of study. It may not be universally known to readers of your Journal that the University of London is an examining and not a teaching University. Its graduates come from all kinds of colleges and they may have been prepared by private instruction. Its degrees are valued for their high standard and the severe tests, which it is admitted, they impose. For the B. A. and B. Sc. pass degress, a very fair knowledge of Psychology, Logic and Ethics was requisite until recently. Now it is optional whether the candidate takes Mental Science or Mathematics. There is a separate examination for Honours in Mental Science; for this, in addition to the above subjects, special books are set each year. The M. A. degree (Branch III) to which graduates in Arts may proceed, provides however the chief Mental Science examination. This includes Logic, Psychology and Ethics, Political Economy, History of Philosophy and Political Philosophy. For the latter two divisions, special books are set each year; for the other subjects no books are prescribed by the University. Science Graduates may proceed to a D. Sc. Degree in Philosophy by a further examination for which an original thesis must be produced.

The University also conducts an examination in the Art, Theory and History of Teaching, for which it confers a Teacher's Diploma; this examination includes a paper on Mental and Moral Science. The present examiners in Mental Science are Dr. James Sully, so well known by his writings on Psychology, and Professor Knight of St. Andrew's University.

Coming now to the teaching Institutions, University College (Gower Street) deservedly stands quite to the front in any estimate of philosophical work. Its students prepare for the University with which the college has been closely associated from its foundation. This is, I believe, the only institution in the Metropolis in which there is any endowment of Philosophy.

Professor Croom Robertson, M. A., the pupil and discriminating disciple of Bain, has for more than twenty years filled the chair of Grote Professor, and numbers among his students many of those who now lecture elsewhere, including the present writer. Professor Robertson is widely known for his philosophical erudition, his cultured lectures, his scientific spirit, and his devotion to the cause of philosophy. His are the only lectures of note on Philosophical Systems and History, which are available to the general public in the Metropolis. Professor Robertson was also the Editor of "Mind" from its inception in 1876 until last year when his health unfortunately compelled him to discontinue that task. The method pursued by Professor Robertson in his Lectures will be best understood from a copy of his general Syllabus extracted from the College Course.

GENERAL COURSE.

Psychology:—Thirty Lectures in *First* Term, beginning October 12th.

Logic:—Thirty Lectures in *Second* Term, beginning January 11th.

General Philosophy; Ethics:—Twenty Lectures in *Third* Term, beginning May 2nd.

The course is primarily designed to meet the requirements of Elementary Students, and more particularly Candidates for the B. A. and B. Sc. Degrees of the University of London. The topics to be treated under the head of General Philosophy correspond with some of those included in the *psychological* and *logical* divisions of the University's scheme of Mental and Moral Science. To make the instruction as thorough as possible, lectures are varied or supplemented by conversation, and are followed up by a regular series of exercises to be written at home.

Students who take the Course with a view to B. A. or B. Sc. Honors or to the M. A. Degree, (Branch III) or for no purpose of examination at all, have their respective needs carefully attended to from the first, their reading being specially directed, and (where necessary) more advanced exercises being prescribed.

SPECIAL COURSES.

History of Philosophy.

First and Second Terms.

Plato (*Theaetetus Republic*) and Hume, as prescribed for the M. A. Degree in 1892.

Third Term.

Plato (*Phaedo*) and Adam Smith (*Moral Sentiments*), as prescribed for B. A. and B. Sc. Honors in 1892.

These Special Courses will be given at times to be arranged privately with the Students concerned. Names for the M. A. Course should be sent in to the Professor by the 16th October; for the B. A. and B. Sc. Honors Course, by the end of the Second Term.

The *John Stuart Mill Scholarship* is open to the competition of Students within two Sessions *after* completion of the General Course.

The subject prescribed for the Mill Dissertation in the Session 1891–92 is "The Development of English Philosophy till Hume."

King's College, (Strand), has also been in close relation with the University from its foundation though it now sends up very few students for Degrees. It is a Church of England College and confers a title of its own (A. N. C.). There are regular courses in Logic and Mental

Philosophy, though Philosophy is not now a very prominent feature of the College Course. The Rev. Frederick Denison Maurice was at one time professor here. The following is a copy of the Syllabus as it appears in the College Calendar:

LOGIC AND MENTAL PHILOSOPHY.—"Lectures are given on these subjects on Wednesdays from two to three P. M. Each course will run through three terms, will consist of about thirty lectures, and is intended to give such a general knowledge of the subject as every educated man may be suspected to possess. The requirements of the B. A. and B. Sc. examinations of the University of London will be constantly kept in view." The Rev. A. Caldecott, St. John's College, Cambridge, is the present professor. There is an evening class on Mondays from seven to eight P. M. The College has also a separate department for ladies at Kensington where a Course of Lectures on the Ethical teaching of English Poets and Essayists of the Nineteenth Century is being given.

Bedford College for Ladies, (Baker Street, W.,) is an institution for the higher education of women. Founded in 1849, it has regularly prepared its students for the University of London since 1879 when the Degrees were thrown open to women, and its curriculum is mainly regulated by the requirements of the University. The College is well appointed and supplied with laboratories and apparatus. The accommodation is excellent and some twenty-five students reside on the premises. A Training Department for Teachers has been recently formed; for this class as well as to the Students for Degrees, Mrs. Bryant, D. Sc., (London), is delivering a course of Lectures on the elements of Psychology; this will be followed by a course in Ethics and Logic.

There are several valuable Evening Colleges in London which provide higher education for persons occupied during the day, and which also prepare for the examinations of the University. Foremost amongst them is the *Birkbeck Institution*, (Chancery Lane), with some four thousand students of both sexes, and classes and lectures on all kinds of subjects from Arithmetic to Astronomy. The writer has for many years lectured here on Logic, Psychology, Ethics, and Political Economy, to numbers of students engaged during the day as City Clerks, Teachers, etc. Some proceed to Degrees at the University, and others to the Cambridge Higher, Women's and other examinations. The principal text books are Jevons, Mill, Bain and Veynes, on Logic; Sully and Höffding on Psychology, and Sidgwick's Methods and History on Ethics. The courses are arranged to cover all the London University examinations in these subjects.

The *City of London College*, (Moorfields), is an exactly analogous Institution. Its curriculum resembles that of the Birkbeck, and with a smaller body of students it carries on work of the same character. Logic, Psychology and Ethics have a permanent place in its Syllabus, and the description just given may be taken as indicating its character and aims.

A comparatively new Institution is the *London Ethical Society*, (Essex Street, Strand), established about five years ago with the object of developing interests in Ethical and Social subjects. This is done primarily by free Sunday evening Lectures followed by discussions; the current programme of Lecturers contains among others the names of Allanson Picton, Felix Adler, and D. G. Ritchie. The Society further organizes courses of week night lectures on Ethical and Philosophical subjects in which it aims at "establishing more systematic teaching in the subjects dealt with at the Sunday lectures." Mr. Muirhead lectures on [illegible] Mrs. Bryant has recently completed a course of Lectures on " [illegible] Life." Mr. B. Bosanquet is now engaged on a course on " [illegible] of Knowledge," and it is proposed next winter to deal with [illegible] osophy of Art," and "History of Religion."

There is another Institution which must be noticed. *Toynbee Hall*, Universities Settlement in the east of London, is probably well known to Americans. There, for some years, a colony of university men has been working in many ways to elevate the tone of east London; the plan has included lectures, reading classes, students' societies, etc. Many eminent men have delivered lectures on Ethical topics, and a Toynbee Philosophical Society has been founded over which various able University men have from time to time presided, including recently Mr. Alexander of Lincoln College, Oxford, whose name is known as a writer on Ethics and Psychology. The labor is of a voluntary kind for the most part, and its primary aim had doubtless much more of a missionary character, though it has developed considerably.

It remains only to speak of the work of Dr. James Sully as a Lecturer on Psychology and Education. For many years Dr. Sully has lectured at the College of Preceptors (Bloomsbury) on the "Science, Art and History of Education." These lectures are the most popular and systematic lectures of the kind in the Metropolis; they attract annually a large attendance of Teachers in Secondary Schools. Their aim as set forth in the the Syllabus is to show that "there are definite truths relating on the one hand to the characteristics and laws of growth of the child, and on the other hand to the ends of human life which have a direct bearing on the Teacher's work."

Dr. Sully's reputation as a Psychologist is too great to need mention here, his books have become text books throughout the world; his experience in the application of Mental Science to the principles and practice of Education is equally extensive with his scientific acquirements in the field of Mental Science, and these lectures are a most important factor in the diffusion of the principles of Psychology in the Metropolis.

Dr. Sully lectures also on Psychology at the Maria Grey Training College, Fitzroy Street, the first Training College for Women Teachers in Secondary or "High Schools." This College was founded in 1878 for the Training in the Theory and Practice of Education of women who desire to devote themselves to teaching in girls' Secondary Schools, and who aim at a University Certificate of professional skill. Dr. Sully further lectures at some of the Normal Schools for Elementary Teachers.

The following very condensed summary of a current Syllabus will give some idea of the plan adopted in Dr. Sully's lectures on Education:

1. Education as a science and an art—its place in relation to human activity, social progress, civilization.
2. The true purpose of education—different conception of the aim or bearing upon perfection, fitness, happiness, knowledge, and moral character.
3. How the educational end is to be realized—by exciting normal reaction in the organism—by self activity, bearing of education upon natural development.
4. Physical education as an end and as a process—healthy development of powers—games, gymnastics, discipline.
5. Education of senses—training of mind organs—awakening of intellect, attention, observation, object teaching, perception—naming and registering results of observation.
6. Transition from sense perception to ideation—image, ideas, naming and reproduction, memory, realization of the unseen—constructive imagination, language and description.
7. Transition from concrete to abstract—generalization—process of thought—definition and induction—aids in mental development from analogies; reasoning.
8. Psychological and logical view of knowledge—order of acquisition—empirical as introductory to scientific knowledge—topical concatenation of studies.

9. Knowledge and particular knowledge—selection of studies and bearing upon development of human faculty—the ideal currriculum.

10. Education as concerned with feelings—calling forth interest by education, enthusiasm, æsthetic culture—formation of taste in literature and in art.

11. Education as acting on will and character—value of method—development of intelligent sense of duty—influence of custom, law, society, etc.

12. Typical plan of education and its concrete modification—the spirit of the age—nationality—adjustment of education to individual needs—specialization, etc.

This brief review of Philosophical Teaching in London takes no note of Societies for discussion like the Aristotelian, or of instruction which is more or less for a private character, or of the University Extension Society, which, except in the cognate branch of Political Economy, has not yet developed Philosophical Study, although the lectures of the Ethical Society have been brought into relation with this Society. It will be seen that in a scattered and disconnected form there is a considerable supply of instruction in some of the branches of Philosophy, under the heads of Logic, Psychology, and Ethics, and to these might be added Political Economy did the scope of the paper permit, but for the study of General Philosophy on a systematic plan, University College is practically the only centre.

The subject of a Teaching University for London has for several years been agitating the public mind. A recent attempt to transform University College, King's College, and the Medical Colleges into such a University to the exclusion of the other teaching institutions has failed. A Royal Commission is being appointed to consider the whole subject and to suggest some plan co-ordinating under one head the scattered agencies of a higher education in the Metropolis. It is confidently hoped that before long some system will be devised by which they will be brought into closer relation with the existing University, or failing that, be organized under a New Teaching University which will systematize their work, stimulate to the utmost their energies and prevent that waste of power which is inevitable in the circumstances when a number of isolated educational bodies follow their own plans with no common bond or directing force.

We may hope that when this project takes definite shape, among other good results will be an impetus to philosophical study, and the full recognition of its bearin g upon education and life. And further we may hope that a Metropolitan University would establish and properly equip a laboratory for experimental psychology and research, such as is to be found on the Continent and in more than one American college, the absence of which can not but be regarded as indicating a very imperfect appreciation of the value of such pursuits in the greatest city in the world.

I am,
Yours faithfully,
G. ARMITAGE SMITH.

COPENHAGEN, April 6, 1892.

TO THE EDITOR,
My dear Sir,

You have in your friendly letter expressed the wish to know something about my work as a teacher of philosophy. There is not much to tell, but perhaps it will be of interest to you and your readers to hear something about the manner in which philosophy and psychology are studied in our little country.[1]

We have the rule at our university, that the students in their first year go through a philosophical course, consisting of four hours per week through two semesters. My colleague, Professor Kromoa, and I conduct this course, so that the students can choose which of us they will hear. In this course I make use of my "Outlines of Psychology," of which the greater part is read every year. I treat Psychology as a fundament of Philosophy, all three great philosophical problems—the problem of knowledge, the problem of being and the ethical problems—being intelligible only from the point of view of human consciousness. Empirical Psychology is thus an introduction to Philosophy. In this spirit is my book conceived. I have endeavored to give a complete view of the facts and forms of psychological life with special stress on the subjects which are interesting from a universal philosophical point of view. I have, so far as possible, endeavoured to make use of all the sources of psychological experience and knowledge. And I have sought to express my thoughts as briefly and clearly as possible. I cannot here omit to say that the friendly reception my book has experienced in England and America, is in a very great measure due to the excellent English translation, for which I am indebted to Miss Mary Lowndes.

In the said course I give further the elements of Logic (after the method of Jevons), and sometimes a few chapters of Ethics or of the history of Philosophy.

Other lessons are designed for those students that are peculiarly interested in Philosophy. Here I discuss philosophical and psychological questions, often in the form of colloquia. Thus I have treated a series of questions on which of late I have written several papers, which in a German translation have appeared in the " *Vierteljahresschrift f. wiss. Philosophie* (vol. XIII—XV). Or I read with my students some philosophical work (e. g. the Ethics of Aristotle; Spinoza's "Ethica;" Kant's Kritik der reinen Vernunft, der praktischen Vernunft, der Urtheilskraft; the Logic of Stuart Mill; Spencer's "First Principles;" Wundt's Logic).

Finally, I lecture on the history of Philosophy or on Ethics. These lectures are attended not only by students, but also by other ladies and gentlemen. The rooms of our University stand open for all who think they can get any profit from the lectures.

In these last years (after the appearance of my "Ethics") my studies have been concentrated on the history of Philosophy.

My colleague, Prof. Kromoa, has as text-book for his course his "Logic and Psychology" (of which a German translation has appeared). His other lectures are on the theory of knowledge (on which he has written a work, which is translated into German: "Das Naturerkennen") and Pedagogics. Dr. Wildens is lecturing on Aesthethics and Sociology, Dr. Starke on Ethics, Sociology and History of Philosophy. We have at our University a psychological laboratory, under the direction of Dr. Lehmann, whose experimental treatises are translated in Wundt's Studien.

This will give you a short view of my "work and surroundings."

Believe me, dear Sir, yours very truly,

HAROLD HÖFFDING.

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[1] For a fuller view see the paper of my late young friend *Knud Ipsen;* "Die dänsche Philosophie des letzten Jahrzehnts" (Phil. Monatshefte, 1891, XXVII, 290.) and my paper in the Archiv fur Geschichte der Philosophie, II.

BERLIN, April 14, 1892.

My dear Professor Stanley Hall:

I feel rather flattered by your kind inquiry about my lectures, but find it difficult to give you an appropriate answer in a short letter. Since I have come to the University of Berlin (1888), I have lectured on subjects which have always specially interested me. The physiology of protoplasm, comparative physiology, general physiology, psychogenesis (in the Victoria Lyceum), physiology of sensation (in the Urania), and macrobiotics.

The titles of my regular lectures in the University as given in the catalogue do not convey an exact idea of their full contents, but I am obliged to give short titles like all the others. This does not in the least prevent me from expounding my own views, for instance on psychology in my lectures on the physiology of hypnotism, the first academical lectures on the subject held in Germany, on the evolution of physiological functions in general and on psychical functions especially in my lectures on "Concurrency" ("die Lehre vom Kampfe um das Dasein.")

Protoplasm being the basis of life is my greatest favorite, and I have been led to investigate this wonderful complex of changing substances with increasing interest, but the necessity of preparing new editions of previous books (Mind of the Child, 4th ed.,) and pamphlets or papers, absorbs a good deal of my time, or rather has done so during 1889–91. I am working hard now to get my work on the organic elements and the generic system of elements in general ready for print. I hope to see you in London August 1, at the Psychological Congress, and at Edinburgh August 3, at the meeting of the British Association. I shall read a paper at London on the origin of the notion of numbers, and send some abstracts, which, although they are not quite new, may be unknown to you and may perhaps interest you.

Yours sincerely,

WM. PREYER.

Monsieur le Prof. G. Stanley Hall, Editeur de L'AMERICAN JOURNAL OF PSYCHOLOGY, *Clark University.*

GENÈVE, SUISSE, 14 Avril, 1892.

CHER MONSIEUR,

Une chaire extraordinaire de *Psychologie Expérimentale* a été créeé l'an dernier dans la Faculté des Sciences de notre université (mais sans laboratoire). Ayant été chargé de cette enseignement, j'ai naturellement jugé indispensable de le compléter par des travaux pratiques. Au moyen de quelques instruments que je possède, et d'une salle que l'État m'a prêtée dans le bâtiment de l'universite, j'ai pu, le 15 février dernier, ouvrir aux étudiants un laboratoire très modeste, dont nous devons encore nous contenter cet été. Pour le semestre d'hiver prochain, nous avons la perspective d'obtenir un local mieux aménagé, composé de cinq petites chambres d'une superficie totale d'environ 100 mètres carrée, où je mettrai les instruments suivants à la disposition des étudiants: chronoscope de Hipp (de Peyer et Fararger à Neuchâtel); chronomètre de d'Arsonval (de Verdin à Paris); pendule marquant les .01 de seconde (d'Elbs à Fribourg $^{1}|_{\mathrm{B}}$); quelques instruments d'optique et d'acoustique, périmètre de Landolt, diapasons d'Appun, etc; divers modèles du cerveau, entre autres le grand modèle d'Auzoux (décrit dans votre journal, tome IV, p. 132); enfin quelques-uns des ingénieux instruments imaginés par M. Münsterberg à Fribourg $^{1}|$B, et que grâce à son obligeance j'ai pu faire reproduire par son constructeur M. H. Elbs.

(Sphygmograph, Augenmaasapparat, Schallapparat, Arbwemegungenapparat.) Quand nous aurons ainsi installé en fait un petit laboratoire de psychologie, l'État ne pourra manquer de le reconnaitre officiellement, et de lui accorder un crédit annuel permettant de lui donner peu à peu un plus grand developpement. En ce qui concerne nos travaux, nous nous bornons pour le moment à quelques recherches élémentaires sur les temps de réaction et d'association, sur les types d'imagination, etc.

Je me souviendrai à l'occasion, cher Monsieur, de votre offre aimable d'insérer de nos travaux dans votre estimé et très-intéressant Journal, dont je suis un fidèle abonné, et un lecteur régulier, depuis sa fondation.

Veuillez, je vous prie, recevoir l'expression de la considération la plus distinguée

de votre bien dévoué,

THÉODORE FLOURNOY.

DORPAT, 11. IV. 1892.

Hochgeehrter Herr College!

In Beantwortung Ihrer geehrten Zuschrift beeile ich mich, die gewünschten Auskünfte Ihnen zu übermitteln. 1. Die Klinik für Nerven- und Geisteskranke verfügt über 80 Betten. 2. Es existirt ein Cabinet a) für mikroskopische, b) für psychophysische Untersuchungen. 3) Ich lese klinische Psychiatrie, 4 Stunden in der Woche, und Poliklinik der Nerven- und Geisteskrankheiten ebenfalls 4 Stunden. Der Besuch der Vorlesungen ist für die Studirenden nicht obligatorisch; auch findet kein Examen statt. 4. Im laufenden Jahre habe ich eine Arbeit, betitelt "Criminalanthropologie" (rüssisch) veröffentlicht. Dr. Daraszkiewicz, Assistent der Klinik, erfasste eine Studie, "Ueber Hebephrenia" (Inauguraldissertation).

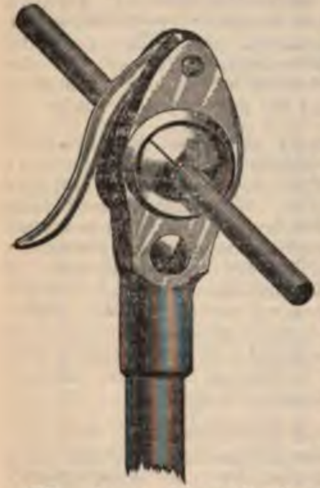
Hochachtungsvoll,

WLADIMIR v. TSCHISCH.

On the Question of Psychophysiology, Consciousness and Hypnotism. In Mind, 1891, XVI, No. 63, E. W. Scripture writes, "The [materialistic] theory asserts that certain of these nervous phenomena produce states of consciousness or mental phenomena and others do not." Materialism is an obscure term. To be sure, such an assertion and a monism understood in this sense would be nonsensical. Researches in hypnotism, however, have strikingly proved that so far as its reminiscent content is concerned our consciousness (subjectivism) depends directly on the phenomena of inhibition or on the absence of such phenomena. Normal dreamlife, natural and artificial somnambulism, all prove that our cerebral activity can divide itself into several varieties or kinds which may be or may not be separated from their objective dynamisms and which appear subjectively as completely independent of each other. The most familiar form of cerebral activity appears to us in the shape of our chief consciousness in the waking condition. This chief consciousness, however, we can, by hypnotic suggestion, insert or cut out at pleasure. The nervous processes which at each moment appear as "unconscious" are really not such, but are only cut out from the momentary chain of phenomena of the chief consciousness, whether it be that they become obliterated for a time, or that a sudden inhibition hinders them from taking part in an association with that chain of phenomena, or that, as is often the case, they take place in another part of the central nervous system of which the subjectivism is mediately and loosely connected with our cerebral consciousness (just as its cell-fiber system is mediately and loosely connected with the cerebral systems), or finally that the process is so short and so weak that even in occurring it is, so to speak, forgotten. According to this view, which agrees essentially with Janet's and Dessoir's "multiple consciousness," we do not need to assume a nervous process that goes on without consciousness. On the contrary we can and must suppose that in all probability consciousness is not only to be attributed to various parts of the nervous system, but exists also outside of the nervous system in the natural world as the simple primitive form of subjectivism. Physiological psychology has consequently to study the correspondence between the phenomena of our chief consciousness (that is, of the field of psychology) with that part of the cerebral activity to which they correspond and therefrom to deduce the laws for other similar correspondence. It thus has to attempt to reduce the psychological phenomena to the laws of the physiology of the nervous system. PROF. A. FOREL.

Hypnotism in the Asylum. In my work on hypnotism I have mentioned that I used suggestion in the noisy divisions of the Burghölzli Asylum for the purpose of making the attendants insensitive during their sleep for the dreadful racket of the patients; thus they can sleep quietly and restfully and yet wake up upon the occurence of any unusual disturbance among the patients. Up to the present the chief noise was on the women's side and this action was not necessary on the men's side. Last summer, however, two new attendants complained of the great noise of the restless male patients and asked me for help. It was sufficient for me to hypnotise them with the appropriate suggestions last June. Since that time they have not heard the noise during the night and have always slept well, although the noise has continued to be very great. PROF. A. FOREL.

O. C. White of Worcester has patented a ball-joint that is exactly what psychologists and physiologists have so long sought. One of the forms is shown in figure 1, where the joint is fixed at the end of a rod. The fastening of the joint takes place with absolutely no variation of the adjustment. The manner in which the parts clamp together is peculiar, being everywhere a wedge-action. The curvature of the inside of the socket is that of a sphere smaller than the ball which it encloses; likewise the hole through which the slide-rod is placed is of smaller radius than the rod. Consequently the parts do not touch over their whole surfaces, which would render firm fastening difficult, and after a little wear, impossible. As a result a slight pressure on the lever serves to cause a good fastening and with full pressure a 2½ inch ball will support 75 pounds on the rod at a foot from the center without the slightest sliding.



The clamp shown in figure 2, is also a universal joint, but the movement is obtained on a different principle. It is arranged to slide on the rod of a stand in the usual way but it contains two discs revolving in a vertical plane, these having a cylindrical opening for the rod to be held. This gives the up and down motion, a complete circular movement in any vertical plane and, by pushing the rod through to the desired extent, the radius of the circle can be of any length.

According to the Yale circular of graduate instruction, early in next year laboratory work in experimental and physiological psychology, under a special and competent instructor, will be opened to graduate students,—comprising the following two courses:

1. *Experimental and Physiological Psychology.* 2 hrs. both terms.

This course will provide for a study (illustrated by charts, models, histological preparations, and a certain amount of laboratory work) of the human nervous mechanism, and of the principal relations which exist between changes in this mechanism and the activities of the mind. The text-book is Ladd's "Elements of Physiological Psychology."

2. *Special Problems in Psychology.*

Under the guidance, and with the assistance, of the instructor, particular problems in experimental and physiological psychology may be worked out in the laboratory. Such work will be permitted to count for the degree of Doctor of Philosophy, according to its excellence and the amount of well-spent time devoted to it. It is expected also that, in certain cases, theses for this degree may be prepared as giving the results of such work.

The psychological instruction at Harvard next year will consist of three courses. The elementary course will be conducted by Prof. Royce on the foundation of James's Briefer Course; it will extend over one-third of the year with three hours a week. The advanced course will be conducted by Dr. Nichols, using Ladd's Outlines and James's Principles and including a thorough course of laboratory exercises; it will occupy three hours a week throughout the year. The graduate course will be in the hands of Prof. Münsterberg, formerly of the University of Freiburg, who is now Director of the Psychological Laboratory; he will have general control of the experimental work.

The Susan Linn Sage School of philosophy, at Cornell, was founded in September, 1891, with the addition of $200,000 to the previous endowment for a professorship. The leading idea seems to be the employment of specialists in each line of philosophy. One instructor devotes all his time to the history of ancient philosophy, and two others attend strictly to the history of modern philosophy along with systematic metaphysics. There is a professor of the history and philosophy of religion and another of pedagogy. Another professor gives most of his time to ethics; there is also a special instructor of logic. *The Philosophical Review* edited by Prof. Schurman is the organ of the school. A department of psychology has also been opened. The laboratory will contain equipment of apparatus most of which was made at Leipzig under the personal supervision of the professor, Dr. Angell.

The following list of lectures and exercises in the German universities last winter is intended to include not only the strictly experimental work but also those courses in pathology that have a distinctly psychological bearing.

Leipzig: WUNDT, (with the assistance of Dr. Külpe), Special investigations and exercises in the psychological laboratory. KÜLPE, Lectures on psychology, Introductory course in the psychological laboratory. GLÖCKNER, Pedagogical psychology. FLECHSIG, Psychiatrical clinic, Forensic psychiatry. *Berlin:* DILTHEY, On the application of psychology to pedagogical questions, Lectures on psychology. LAZARUS, Lectures on psychology. EBBINGHAUS, Lectures on psychology with reference to experimental and physiological psychology, Exercises in experimental psychology. JOLLY, Pathology and therapeutics of mental diseases, Clinic. *Bonn:* MARTIUS, Elements of psychology, Psychological exercises. PELMAN, Mental disturbances that border on insanity, Clinic. KOCHS, On hypnotism, sleep and the narcotic condition. *Goettingen:* G. E. MÜLLER, Lectures on psychology, Experimental psychological investigations. MEYER, Psychiatrical clinic. *Heidelberg:* KRAEPELIN, Physiological psychology, Psychiatrical clinic.

The *Institut Psycho-Physiologique de Paris* was founded in 1891 for the theoretical and practical study of the psychological and therapeutical applications of hypnotism. A free clinic for nervous diseases is annexed to the institute where physicians and students of medicine regularly inscribed are admitted for practice in psychotherapy. A private hospital adjoining the clinic receives morphiomaniacs and those requiring constant attention.

The *Société d' hypnologie* of Paris has monthly meetings, at which papers on hypnotism are read and clinical cases presented. Dr. Dumontpallier is president and Dr. Bérillon is general secretary.

The Second Session of the International Congress of Experimental Psychology will be held in London, on Tuesday, August 2d, 1892, and the three following days.

Arrangements have already been made by which the main branches of contemporary psychological research will be represented. In addition to the chief lines of investigation comprising the general experimental study of psychical phenomena in the normal human mind, it is intended to bring into prominence such kindred departments of research as the neurological consideration of the cerebral conditions of mental processes; the study of the lower forms of mind in the infant, in the lower races of mankind, and in animals, together with the connected laws of heredity; also the pathology of mind and criminology. Certain aspects of recent hypnotic research will also be discussed, and reports will be given of the results of the census of hallucinations which it was decided to carry out at the first session of the Congress (Paris, 1889).

On Feb. 12th died Hermann Aubert; born November, 1826, at Frankfort a. O., he studied chiefly in Berlin, taking his degree in 1850. Appointed Professor of Physiology at Rostock in 1865, he devoted himself mainly to physiological optics. After a smaller work, the *Physiologie der Netzhant* (1865), he published his famous "*Physiologische Optik*" (1876). His last experimental investigations referred to the limits of accuracy of ophthalmometric measurements.

The publication of an encyclopedia of medical propædeutics is about to be begun under the direction of Prof. Gad in Berlin. Extensive space is to be given to physiological psychology and the neighboring subjects. The psychology of the senses, excluding sight, is to be treated by Goldscheider, physiological optics by Cl. Dubois-Reymond, cerebral physiology by Ziehen and general psychology by Münsterberg.

At the University of Basel died the eighty year old Prof. I. Hoppe, formerly practicing physician, but for many years busied with psychological speculation. He has bequeathed half a million marks to the university, with the condition that a commission shall be appointed which shall in Hoppe's house meditate uninterruptedly on the nature of the soul, and shall in publishing their results refrain from the use of all foreign words.

The laboratory of experimental psychology of Columbia College is established in four rooms, occupying the upper floor of the president's house. These include rooms for instruction and research, and a dark room for the study of vision. A collection of apparatus has been secur
at a cost of about $2,500, and this will be further increased during t

present year. The liberal regulation recently adopted by the trustees makes it possible for men of science not connected with the college to use the laboratory and apparatus for special research.

According to statistical material gathered by Volkelt, the lectures on psychology have increased most of all in the universities of German tongue. During the year 1887-88, the lectures were distributed as follows: Psychology 67, logic 64, metaphysic 25, ethics 41, history of philosophy 154; whereas in the year 1890-91, there were: Psychology 78, logic 64, metaphysic 27, ethics 51; history of philosophy 138.

A free course of lectures on the clinical and medico-legal applications of hypnotism is given by Dr. Bérillon in the Practice School of the Faculty of Medicine at Paris.

Dr. Max Bessoir, well known for his writings on hypnotic subjects, has been appointed Docent of Philosophy at the University of Berlin. He will lecture chiefly on psychology and æsthetics.

Professor Flournoy has been appointed to the newly founded chair for physiological psychology at the University of Geneva. A laboratory is about to be begun on the plan of that at Freiburg.

The psychiatrist and psychophysicist, Prof. Kräpelin, who was recently called from Dorpat to the medical faculty of Heidelberg, has begun a laboratory for experimental psychology. Psychological lectures will be given in addition to the psychiatrical ones. The University of Dorpat will continue the courses in psychology under the direction of Kräpelin's successor, Woldemar von Tschisch, the Petersburg psychiatrist. Both Kräpelin and Tschisch are former pupils of Wundt.

The third Congress of Criminal Anthropology will be held at Brussels from the 28th of August to the 3d of September of this year. The extensive program includes nineteen groups of subjects to be considered. Communications are to be addressed to M. C. Dr. Semal, président, l'Asile de Mons, Belgique.

A grant of £250 has been made to the Physiological Laboratory of the University of Cambridge, England, for the purpose of establishing a psychological department. This is the only opportunity for psychological instruction in the laboratory in England.

A laboratory has been established in the University of Toronto. Its fittings have cost about $450. An appropriation of $800 has been made for apparatus, and $300 a year have been allowed as for maintaining it. Prof. Baldwin has gone to Europe largely in the interests of equipment.

Prof. Frank Angell of Cornell has accepted the chair of psychology at the Stanford University.

Dr. Edward Pace, a pupil of Wundt, who took his degree at Leipzig with a dissertation entitled "Das Relativitätsprincip in Herbert Spencer's psychologischer Entwicklungslehre," has taken the chair of psychology in the Catholic University at Washington and has started a laboratory.

Edmund Delabarre, Ph. D., (Freiburg), has just gone to Brown University after taking his degree with an experimental investigation made in Münsterberg's laboratory. A laboratory will be started.

Dr. Charles Strong will lecture next fall at the University of Chicago.

American workers are to be again warned against importing apparatus which is wholly or partly made of wood. In a few months the American climate warps the wood and consequently renders the apparatus, in most cases, almost useless. This fact, long since known to piano-dealers, is sometimes lost sight of in the laboratories or is not learned till after expensive experience.

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THE EXTENT OF THE VISUAL AREA OF THE CORTEX IN MAN, AS DEDUCED FROM THE STUDY OF LAURA BRIDGMAN'S BRAIN.

HENRY H. DONALDSON, PH. D.,
Assistant Professor of Neurology in Clark University, Worcester, Mass.

Some peculiarities of the cerebral cortex, in the case of Laura Bridgman, have already been described in this JOURNAL ([1, 2]).

It was there pointed out that the three localities examined within the occipital lobe had the cortex thinner in the right hemisphere. The optic nerve and tract belonging to the left eye were the thinner, and in the right cuneus, the gyri were irregular. These facts all pointed to a greater disturbance affecting the right occipital lobe. Since in the left eye vision was completely lost at the age of two years, while it persisted up to the eighth year in the right, the thinness of the cortex on the right side was explained by the earlier loss of vision in the left eye, a lesion which was assumed to have arrested its development. Since the completion of the papers mentioned above, a further consideration of the case suggested that this brain might be used to determine the extent of the visual area in man. In following out this suggestion it has been assumed: (1) That the thinning of the cortex was due to an arrest of development; (2) that this thinning would extend over the entire visual area; (3) that, in the regions compared, the disturbance in vision was the principal influence acting to arrest unequally the growth of the cortex;

(4) that the cortex would be most thinned upon the side of the brain opposite to the eye and nerve most affected; (5) that the visual area in the normal brain gradually merges into the surrounding areas, and that hence the cortex on the outer limits of this area would, in this case, be but slightly different on the two sides.

The observations on which my conclusions are based were made in the following manner:

On a model of the brain, a boundary line was marked off as follows: Beginning at the mantle-edge on the dorsal surface the boundary passed laterad from the mesal end of the sulcus lying next caudad to the superior retrocentral sulcus, to the dorsal end of the ascending ramus of the first temporal sulcus, following this ventrad to its junction with the first temporal sulcus, and thence took the shortest path over the lateral and ventral surfaces, to the most cephalic end of the calcarine fissure, and there passed dorsad to join the mantle-edge at the point of departure. There was thus cut off a pyramidal portion of the hemisphere, with the plane of its base determined by this boundary, and having the occipital pole for its apex.

For the purpose of comparison, this surface was first divided into six areas, and ultimately, by subdivision, into ten areas. The following is a description of the areas.

I. The superior-parietal area. Bounded mesad by the mantle-edge, laterad by the interparietal sulcus, cephalad by the sulcus next caudad to the superior retrocentral sulcus, caudad by the anterior occipital sulcus.

I. was subdivided into a cephalic portion, I. (a), lying cephalad to the cephalic stipe of the interparietal sulcus, and a caudal portion, I. (b), lying caudad to the same.

II. The area of the angular gyrus. Bounded mesad by the interparietal sulcus, laterad by an arbitrary line uniting the junction of the first temporal and its ascending ramus to the transverse occipital sulcus, cephalad by the ascending ramus of the first temporal sulcus, and caudad by the anterior occipital sulcus.

II. was subdivided into a cephalic portion, II. (a), lying cephalad to the ascending ramus of the second temporal sulcus and a caudal portion, II. (b), lying caudad to the same.

III. Area of the cuneus. Bounded dorso-cephalad by the parieto-occipital sulcus, ventrad by the calcarine fissure, and caudad by the mantle-edge.

IV. The area of the occipital pole. Bounded mesad by the mantle-edge which separates it from the cuneus, laterad and cephalad by the anterior occipital sulcus, caudad by the lateral occipital sulcus, and an arbitrary line joining the mesal end of the latter with the calcarine fissure.

IV. was subdivided into a lateral portion, IV. (a), lying laterad to a secondary sulcus which runs through the middle of this area, parallel to the mantle-edge, and a mesal portion, IV. (b), lying mesad to the same.

V. The praecuneal area. This includes that part of the praecuneus which lies cephalad to the parieto-occipital sulcus, and caudad to the boundary line.

VI. The ventral area. Bounded mesad by the calcarine fissure, laterad and caudad by the lateral occipital sulcus and its arbitrary continuation to the junction of the first temporal sulcus with its ascending ramus, cephalad by the base line.

VI. was subdivided into a mesal portion, VI. (a), lying between the calcarine fissure and the fourth temporal sulcus, including, however, only the dorsal two-thirds of the gyrus lingualis, and a lateral portion, VI. (b), lying laterad to VI. (a).

The brain itself was now compared with the model. The portion corresponding to each one of the ten areas just enumerated was cut into a number of blocks, and the position of each block was carefully marked on the model. The block was then mounted and the thickness of the cortex determined. There were sixty-two such blocks for each hemisphere.

The thickness of the cortex was determined in the following manner: The block was imbeded in celloidin and cut into sections about .05 mm. thick, stained with van Gieson's Picro-Fuchsin and mounted in the usual way.

By this stain the gray matter was colored a much deeper red than the white and thus well differentiated from it. The thickness of the cortical layer was measured with an eyepiece micrometer, each division of which had the value of .067 mm.

The following is the detail of the method of measurement: By means of a camera lucida each specimen was drawn on a card, being enlarged about six diameters. The actual length of the cortex in each specimen was next obtained by direct measurement. The thickness of the cortex was measured at the bottoms and sides of the sulci and at the summits of the gyri. Whenever any of these localities was sufficiently extended, more than one measurement was taken. In general the measurements of the thickness of the cortex were made at intervals of 5 mm.

Each measurement was then recorded at the corresponding point on the drawing of the specimen, and by this means any observation could be easily verified.

The above was all done by my assistant who was not informed of the purpose of the investigation.

The next step was to cover all the labels, both on the specimens and the drawings. The drawings were then shuffled and the specimens arranged in the final order in which the drawings stood. In this way the specimens from the right and left sides were mixed together and all order among them destroyed. All the measurements were then repeated by myself. The figures thus obtained were final and have not been altered.

As a result there was obtained for each section a length of cortex and a certain number of measurements giving the thickness of the cortex at about equal intervals of 5 mm. The average of the measurement for thickness multiplied into the length represented the area of the cross-section of the cortex in the given block. In order, however, to compare these areas with one another, it was necessary that the length of cortex should be the same on both sides. As this was rarely, if ever actually the case, the adjustment was made by always choosing as a norm the length of that side where the cortex was shortest. In comparing symmetrical points the difference in the area is thus rendered dependent upon the average thickness of the cortex alone.

In making up the tables from these observations it has to be kept in mind that if from one block in a certain area we have five or ten times the length of cortex that is obtained from its neighbor, that then the average derived from the measurements of this longer cortex applies to five or ten times as much of the region from which it is taken and hence should enter into the sum from which the average is derived five or ten times to its neighbor's once. In making the tables, therefore, the average thickness of the cortex for each block is multiplied by the length of cortex to which it applies. The areas thus obtained are added together and the sum divided by the number representing the sum of the several lengths of cortex. The quotient represents the average thickness of the cortex for the area in question, the thickness as determined for each block having thus been allowed its proportionate weight. Since the length of the cortex was very different in the different blocks the length in that block which had the shortest cortex was taken as a standard. The minimal length was just 5 mm. This length of 5 was taken as a unit, and all the other lengths have been written in the tables as multiples of this unit. In other words those numbers represent the true length of the cortex divided by 5. Since the reduced lengths enter as factors into the columns in which the areas are given, the figures there also represent the true areas divided by 5. With this explanation we present the first table.

TABLE I.

To show the average thickness of the cortex in the several subdivisions, with the absolute difference between the two sides and thickness of right side in percent.

AREA.	No.	Number of Blocks.	Length of Cortex divided by 5.	Area of Cortex in section, divided by 5.		Average thickness in mm.		Left side standard.		
Name.				Left.	Right.	Left.	R'h't	Absolute difference in mm.	Percentage thickness of the right side.	
Superior parietal.	I. a.	2	9.1	24.68	24.49	**2.71**	2.69	+.02	99.2	excluded.
Superior parietal.	I. b.	3	12.5	37.27	34.32	**2.98**	2.74	+.24	91.9	
Angular gyrus.	II. a.	5	20.5	58.09	56.97	**2.83**	2.78	+.05	98.2	
Angular gyrus.	II. b.	6	20.2	61.15	53.30	**3.02**	2.63	+.39	87.0	
Cuneus.	III.	11	38.7	82.69	79.13	**2.13**	2.04	+.09	95.7	
Occipital pole.	IV. a.	6	19.1	42.79	41.83	**2.24**	2.19	+.05	97.7	
Occipital pole.	IV. b.	3	14.8	32.76	31.93	**2.21**	2.15	+.06	97.2	
Praecuneus.	V.	4	19.6	53.56	58.97	2.73	**3.00**	—.27	109.8	excluded.
Meso-Ventral.	VI. a.	6	16.4	37.50	33.41	**2.28**	2.03	+.25	89.0	
Ventral.	VI. b.	17	60.1	140.82	148.37	2.34	**2.46**	—.12	105.1	excluded.

EXPLANATION OF TABLE I.

In this table are entered the numbers for the ten subdivisions. The numbers for the larger areas, e. g. I., etc., are not entered because in the presence of the subdivisions they are superfluous. They can, moreover, be obtained in any case by summing the numbers given for the component subdivisions.

Taking the columns of the table from left to right we have the following data:

(1) Name of area, (2) numerical designation of the subdivision, (3) the number of blocks cut from the subdivision, (4) the total length of the cortex in said blocks, divided by 5, as previously explained. This length has been adjusted so

that it is equal on both sides of the brain. The product of this last number into the number representing the average thickness of the cortex in this subdivision, first for the (5) left, then for the (6) right side. The number in millimeters, representing the average thickness of the cortex in the given subdivision, first for the (7) left, then for the (8) right side. The larger number of each pair in these two columns is printed in heavy faced type. (9) The absolute difference in millimeters between the pairs of figures in the last two columns, showing by how much the number for the left side exceeds that for the right. (10) The thickness of the right side expressed as a percentage. The thickness of the left side being taken as a standard i. e., equal to 100%. (11) Finally, "excluded" is put after each subdivision which can not be considered as part of the visual area.

The numbers for each subdivision as entered in Table I. have been derived from detail tables, giving the length of cortex and average thickness of the same for each block belonging to the subdivision. These detail tables would be worth publishing only in case they were accompanied by figures showing exactly the point at which each section was taken. At present that is not practicable, hence they are not given.

An examination of Table I. shows that in two subdivisions the cortex on the right side is thicker than on the left. Since we are determining the extent of only that cortex on the right side which is thinner, these two cases, V. and VI. (b), are to be at once excluded. In the remaining eight cases the thickness of the right side is from 99.2% to 87.0% of that of the left. The next question, therefore, is what difference may be considered significant for our purpose.

To this end we must determine first, what variations occur in the thickness of the two sides of normal brains.

I have determined the relative thickness of the occipital cortex in the brains of six males and three females, sampling each hemisphere at three points (2, p. 62.).

Conti(3) has determined the same for seven males and four females. The figures apply to his post-rolandic region, namely all that which lies behind a vertical plain passing through the mesal end of the rolandic fissure.

Franceschi's(4) observations were made on ten male and ten female brains and appear to apply to the occipital lobe of Ecker.

In general, then, the figures obtained by Conti and Franceschi are comparable with those which I have obtained from these last measurements of the Bridgman brain. The averages from the controls measured by myself are taken from three localities only, and may, therefore, be expected to show greater variations than the figures of the Italian observers. The following, Table II., shows the thickness of the cortex in the left and right occipital lobes of the normal brain, giving the thickness of the right side as a percentage of that for the left.

TABLE II.

			MALES.				FEMALES.			
				Average thickness of Cortex in mm.				Average thickness of Cortex in mm.		
Authority.	Date	Locality examined.	No. of Brains.	Left.	Right.	Percentage thickness of right side.	No. of Brains.	Left.	Right.	Percentage thickness of right side.
Conti.	1884	Post Rolandic Region.	7	2.08	2.06	99.0	4	2.03	2.03	100.0
Franceschi.	1886	Occipital Lobe.	10	2.28	2.29	100.4	10	2.16	2.14	99.0
Donaldson.	1891	Three points in Occipital Lobe.	6	2.65	2.62	98.8	3	2.48	2.54	102.4
						Average, 99.5				Average, 99.8

Table II. shows the greatest difference between the right and left sides in the case of the six normal males examined by myself, the right side being the thinner, 98.8% of the left. In the females the right side is less thin, averaging for the seventeen cases 99.8%, while for the twenty-three males it is 99.5%.

Taking the greatest difference found, 98.8%, and applying it to Table I. we exclude area I. (a), since there the right side differs from the left less than in the case of the normal males just cited.

The other differences shown in Table I. we may now regard as significant and employ them for plotting the visual area. See Plate I.

According to these figures the outline of the visual area is the following: Commencing where the cephalic stipe of the interparietal sulcus cuts the mantle-edge and passing laterocephalad along the latter to its junction with the inf retrocentral sulcus, the boundary then takes the shortes to the ascending ramus of the first temporal sulcus, following this to its union with the sulcus, from here the shortest line to the lateral occipital sulcus from the mesal end of which an arbitrary line turns toward the fourth temporal sulcus, running parallel to this sulcus it cuts the gyrus lingualis so as to leave the ventral third of the latter in connection with the fourth temporal sulcus and continues to a point just ventrad of the cephalic end of the calcarine fissure, which it joins by an arbitrary line running dorsad, it then passes caudad along the calcarine fissure to the junction of the same with the parieto-occipital sulcus, and finally along this sulcus to the mantle-edge, then cephalad along the latter to the point of departure.

Such are the limits of visual area, as determined by this method, in the right hemisphere of the Bridgman brain.

All previous direct determinations of this area in man have been by the method of limited lesions, and I find that my outline follows so closely that given by Gowers(5) in his diagram and obtained by the latter method, that I venture to think that the two methods lead to almost identical results and hence mutually support one another.

The visual area here outlined can, however, be examined in detail with advantage. The areas of the cuneus and that of the occipital pole [III., IV. (a) and IV. (b)] show the cortex on the right side only slightly thinner than on the left. The areas immediately around those just mentioned, viz., I. (b), II. (b) and VI. (a), show the greatest thinning on the right side. Beyond this second series we have but one other area, II. (a), in which the difference is again small.

The theoretical explanation for this relation of things is the following: The cuneus and the occipital pole form the more fundamental portion of the visual area, hence would be earlier developed and more resistant to disturbing influences. If such were the case the differential effect of the lesion would be less evident in this region because the growth was more nearly complete at the date of the injury. Moreover, both eyes are represented on each side in the areas of the cuneus and occipital pole, and hence the loss of sight in the left eye would have produced some arrest on the left side, though we should expect the arrest to be decidedly less than on the right. So much by way of explaining why the regions of the cuneus and occipital pole do not show greater differences in the two sides.

To explain why the surrounding region does show greater differences, I assume, first, that this region reaches complete development later, and would, therefore, be more affected under these conditions. Where the greatest difference occurs, namely, in the caudal portion of the angular gyrus, II. (b). We have a region which is held by some authors to be in connection with the opposite eye mainly, and hence we have here two conditions which would induce the greatest difference in growth between the two sides. In the outermost area, II. (a), I look upon the visual representations as decreasing, hence the loss of vision would produce a less evident arrest here.

It is hardly desirable to further dwell on these explanations, since, in part at least, they can be tested, and it can thus be determined whether they are valuable.

In this connection there is one remaining observation to be noted. The mounted specimens of the cortex all show to the naked eye a clear stratification due to a light line. If the specimens are sorted according to the distinctness of the light line, disregarding the labels, and we put by themselves those in which the line is clear and sharp, it is found that we have all the samples of the cortex from the cuneus, the occipital pole, and also from the affected part of the gyrus lingualis.

It thus happens that the line of Baillarger or of Gennari, as it has been rechristened by Obersteiner, is co-extensive in this brain with portions of the visual area, which I have called fundamental, but also runs over to the gyrus lingualis, which can not be included in that part for the reasons above given. So evident a structural peculiarity must have some physiological significance, but this case as it stands, does not show what that is.

We conclude then that in this single brain we have the entire visual area marked out. This area includes the cuneus and angular gyrus, but does not pass on to the ventral surface. The thinning of the cortex is not the same throughout the visual area, but is small in the cuneus and occipital pole, large in the areas immediately surrounding it, and, finally, small again in the most outlying portions. The explanation which I have offered for this, is the stage of development in which the various portions of the cortex were found at the time vision was lost, and the degree to which each eye is represented in several portions of both areas. Finally, Gennari's line almost coincides with the fundamental portions of the areas, but oversteps it so far as to take in the dorsal portions of the gyrus lingualis, and thus, at present, cannot be explained as a peculiar character of the fundamental portion.

The idea involved in this investigation is n[illegible] The observations of Huguenin(6), Burckha[illegible] Mills(9) were based upon it, but t[illegible] time that advantage has been take[illegible] ply it in detail.

To myself the point of most interest is that, if these conclusions are warranted, we have now, through the early destruction of sense-organs and the subsequent examination of the cortex, a means of experimentally determining in animals the limits of the several sensory areas. For the feasibility of this plan, the experiments of v. Gudden and his school already offer some indirect support.

EXPLANATION OF PLATE I.

This plate has been made in the following way: On the right side the caudal portion of the right hemisphere is represented in some detail. There are three views: Fig. 1, lateral; Fig. 2, caudal; Fig. 3, mesal. The reversed outline of this same portion is represented on the left side of the plate. The boundary of the portion taken is marked by a heavy line. On the left side the limits of the subdivisions are all marked by broken lines, and within each subdivision is given its numerical designation and the average thickness of the cortex, in millimeters. These are repeated on the right side, but the broken lines are omitted. As will be seen, in certain cases, some of these data are omitted. No ventral view is given because the cortex there is excluded. The excluded portions, so far as shown, are left white, but on the right side the less affected subdivisions are hatched with a single line, while the more affected are so indicated by a doubly hatched line. The hatched portion on the right side, therefore, represents the visual area as determined.

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Plate I.

Showing three aspects of the occipital lobe.

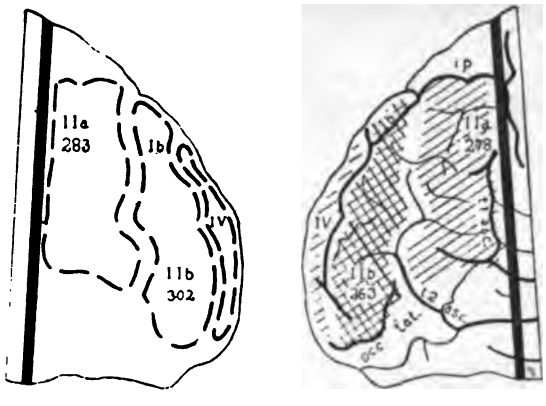

Fig. 1. Lateral aspect.

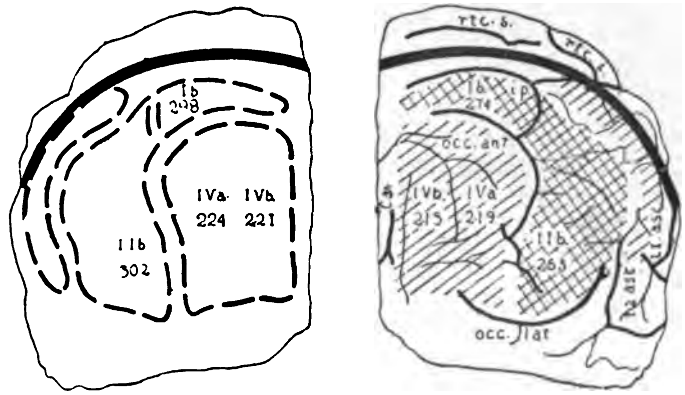

Fig. 2. Caudal aspect.

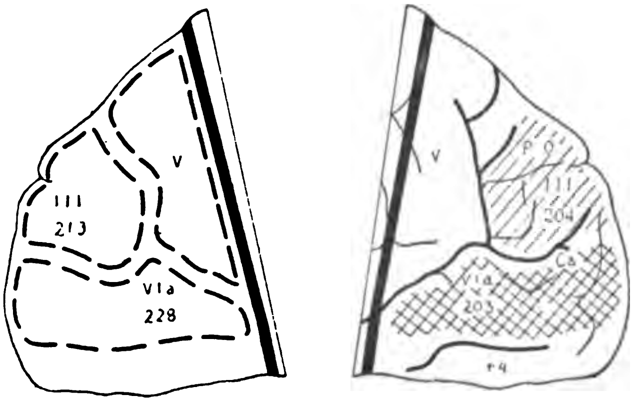

Fig. 3. Mesal aspect.

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SOME INFLUENCES WHICH AFFECT THE RAPIDITY OF VOLUNTARY MOVEMENTS.[1]

By F. B. DRESSLAR, Fellow in Psychology in Clark University.

The purpose of this research was (1) to find a convenient and accurate method of recording and timing rapid voluntary movements, and (2) to determine some of the conditions which influence the rate of such movements. The experiments were limited to a study of the rate at which one can tap on a Morse key. Three hundred taps were made as rapidly as possible at each test, and the condition of the subject noted. During the course of the experiments over 400 tests were made, involving more than 120,000 taps.

Apparatus. The apparatus used consisted of a kymograph with the revolving drum set perpendicularly, and covered with smoked paper. On a standard was fastened an electro-magnet which was put in a circuit alternately made and broken by a clock. Attached to the armature of this magnet was a projecting metal point which registered the seconds on the revolving drum. On the same standard was fastened a common clock movement, the escapement of which was alternately raised by an electro-magnet at one end, and by a resisting spring at the other, according as the electric circuit was made or broken by a Morse key. Thus with each tap on the key the escapement wheel was permitted to move one notch. To prevent jars this key was placed on an adjacent table, but near enough for the operator to manage the kymograph. Upon this key the taps recorded were made. In order to register the taps on the slowly moving drum, a silk cord was passed once around a small pulley substituted for the second hand of the clock. One end of this cord was made fast to a wire bearing a fixed writing point, the other to a counterweight. The wire was held in a perpendicular position by passing it through two brass strips extending out from the board on which the clock was fastened. The writing point was fixed on the wire between these strips and the amplitude of its movement made correspond exactly to 300 taps on the key, by adjusting a wooden clasp on the upper strip. When the drum was moving, the line traced by the writing point formed an angle with the abscissa which varied according to the rapidity of the taps on the key.

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[1] The work was done under the direction of Dr. Warren P. Lombard, Assistant Professor of Physiology, to whom I am indebted for many suggestions in determining methods for work, and for help in devising apparatus.

To determine the time required to make 300 taps, perpendiculars were drawn to the seconds line from the points where the writing point began to rise and where it reached the upper limit of its motion. The seconds comprehended between these perpendiculars gave the time.

The following is an example of a normal record.

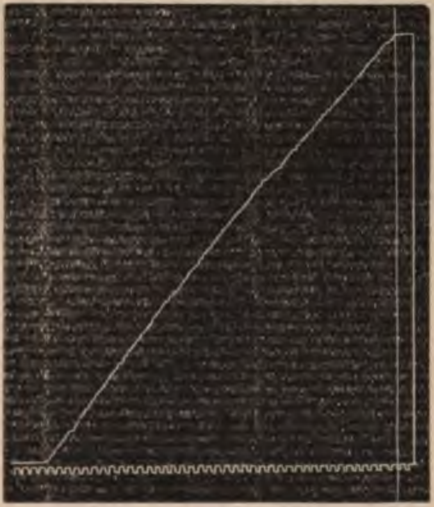

FIG. 1.

Method of Work. From 8 a. m. to 6 p. m. six tests of 300 taps each were made; one every two hours. The usual working hours of the day were thus covered by the experiments, and sufficient time intervened between the records for the subject to recover from any possible fatigue. Three hundred taps were chosen because it was thought that after practice this number would not be fatiguing[1] and at the same time sufficiently great to show any fluctuation in the normal rate. The course of the experiments furnished no evidence that this number was not well chosen.

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[1] It was noticed after the tests had all been made that many of the curves showed a decreasing rate towards the end of the 300 taps and thus gave evidence of incipient fatigue, though all sensations of fatigue had ceased with the first few days of the work. By actual count out of one hundred curves taken at random from the last month of work, seventy showed the effect of this unconscious fatigue. This strengthens the belief that the method used may prove useful as a mild but sensitive clinical test.

The fore-arm rested on a firm support and the tapping movements were as far as possible confined to the wrist. Throughout these experiments the subject gave his whole attention to the work, and made the 300 taps with the greatest possible rapidity. The condition of the operator was carefully recorded beneath each record, and especial attention given to any circumstance which it was thought might influence the rapidity of the taps.

Normal rate of voluntary taps. Table I, column 1, shows that the average rate attained for 300 taps for the time there recorded was 8.5 taps per second. Column 2 should not be included in the average, because during this period many conditions were introduced that more or less affected the normal rate. The most rapid rate attained for 300 taps for any single record was 10 taps per second. Even this last rate could be excelled for a short time. From 30 tests made at the beginning and near the end of the work, the rate for the left hand was found to be 5.3 taps per second.

TABLE I.

No. of seconds required to make 300 taps.

1.							2.						
DATE.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.	DATE.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.
Feb. 3	37	38	36	35	34.5	36	Mar. 10	44.25	37	38	39	34	36
“ 4	37	38	33.5	35.5	32.5	36	“ 11	37	34.5	38	42	32.5	36
“ 5	40	35.5	34.5	35.25	34.5	34.5	“ 12	43	41	36	40.25	36	40
“ 8	39.5	34	33.25	36	33.5	34.5	“ 14	42	43	38.5	43	40	43.25
“ 9	37	32.5	34	36.5	37		“ 15	44	39	39	42	39	46
“ 10	39.75	37	34.75	35	34	33	“ 16	45	38	38.5	41	34	43
“ 11	38	36.5	36.5	34.5	36	37	“ 17	42	39.5	40	41.75	39	40
“ 12	40.5	37.5	37	38	33	36	“ 18	43.5	40	44	40	37	38
“ 13	37	38.5	37.5	39.5	34.5	37	“ 19	39	37.5	36.5	40	36	38
“ 15	38.75	31.25	31.5	30	30.5	32	“ 21	40.25	40	39	40	36	42
“ 16	33.5		33.5	33.5	31.25	38.25	“ 22	41	41	37.5	40	36.25	38
“ 17	36.5	33	32	32.75	32	34	“ 23	45	38	39	37.25	37	39
“ 18	36	35.25	36.5	37.5	33	33	“ 24	40	43.75	41	40.5	38	48
“ 19	39	34.5	33.5	37.25	32	35	“ 25	55	47.5	37.25	41.25	36.5	37
“ 20	37	35	35	36.5	34.5	33	“ 26	40.5	40.5	40	40	38.5	39
“ 22	40.75	36	38.5	33			“ 28	42.5	38.5	42.5	43	41	41.5
Aver.	37.8	35.4	34.6	35.5	33.5	35.1							

General average 35.3.

The rapidity with which the taps were made, at first suggested that the movements were not voluntary in the stricter sense, but more in the nature of tremor. To test this, the contacts of the Morse key produced by normal taps, were recorded on a revolving drum by a Deprèz signal, and each movement timed by a tuning fork of 100 V. D. These were compared with records taken in the same way for tremor
the wrist, and also of the fore-arm. All attempts to p
tremor of the wrist resulted in lateral movements ins
the up and down movements used in making the reg

This fact, together with the greater rapidity of the tremor attained in both cases, seems to leave no doubt that the taps made were separate voluntary movements. Figure (2) shows the contrast (*A*, *B*) between the usual taps and tremor of the fore-arm, and (*A′*, *B′*) between the usual taps and tremor of the wrist. It will be seen from this figure that during short intervals the subject could make 10.5 taps per second, while the rate of the oscillations of the fore-arm during tremor was 12.2 per second, and for lateral tremor of the wrist 12.9 per second.

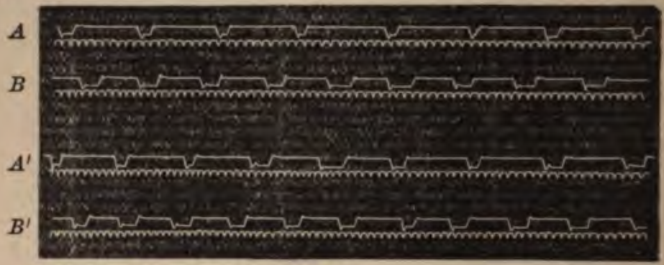


FIG. 2.

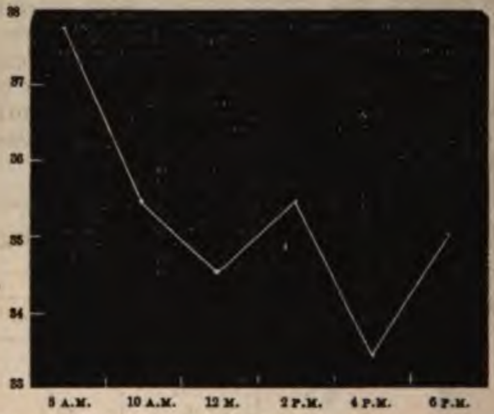
A and *A′* = Voluntary taps.
B and *B′* = Tremor; B of fore-arm, and B′ of wrist.

Voluntary Movements.			Tremor.		
Time in hundredths of a second.			Time in hundredths of a second.		
Line	No.	Time.	Line.	No.	Time.
A	1	9.8	B	1	8
"	2	9.2	"	2	8.5
"	3	9	"	3	7.75
"	4	10.5	"	4	8.8
"	5	9.6	"	5	7.8
"	6	8.1	"	6	8.4
"	7	10	"	7	7.5
A′	1	9.4	"	8	8.75
"	2	10.2	B′	1	6.8
"	3	8.9	"	2	8.2
"	4	9	"	3	8.3
"	5	9.5	"	4	7.8
"	6	10	"	5	7.5
"	7	9.5	"	6	8
"	8	10	"	7	6.7
			"	8	8
Average.		9.5	"	9	8
			"	10	8
			Average.		7.9

Daily rhythm. After six weeks of work it was seen that the daily records indicated a daily rhythm and, when no
varying influences were introduced, the rec
responding hours of each day were quite
ages of the time required to make
hours on the days given in Table I, c

Time of day.	8 A. M.	10 A. M.	12 M.	2 P. M.	4 P. M.	6 P. M.
Time in seconds.	37.8	35.5	34.6	35.5	33.5	35.1

The following characteristic curve is obtained from these averages. This curve would be much less significant did it not correspond very closely in shape to the normal daily curve.



Note.—Distance along the abscissae represents time of day; along the ordinates the number of seconds required to make 300 taps.

The shape of this curve was unexpected, for it was naturally thought that the greatest rapidity would be attained sometime during the forenoon. It will be shown later that the rate at which it was possible to tap was increased by mental excitement or activity, This fact is in harmony with the daily rhythm. The activity of the central nervous system probably increased during the hours of work from 8 to 12, lessened during the noon hour of relaxation, increased again until 4 o'clock in the afternoon, and then, the chief work of the day being over, again decreased. The close correspondence of this curve to the daily programme of work for two years previous is suggestive in the direction of habit; these were spent in public school work with a daily programme beginning at 8 a. m. and closing at 4 p. m., with an hour and a half intermission at noon.

It is probable also, that corresponding changes in the tonus of the muscles of the body, accompanied the changes of central activity, and this increased muscular tension mechanically favored the rapidity of the movements. The decreased rate at 2 p. m. cannot be accounted for by the walk to and from meals, as special experiments showed that the rate was not affected by such short walks. The rise at 6 p. m. may partly be due to fatigue, but it is thought to be chiefly due to an unconscious relaxation, as the chief work of the day ended at 4 p. m. By a careful study of the barometric charts extending over the whole series of experiments, it was found that no effect of atmospheric pressure could be detected.

Effect of Muscular Exercise. After almost two months of work it was noticed that irregularities were introduced into the daily curve when a vigorous walk had been taken. To test this, walks were purposely taken between different periods of work for a series of days; the following table shows the average results; these averages would be less significant, did not the individual cases show every time a decreased rate after the walk:

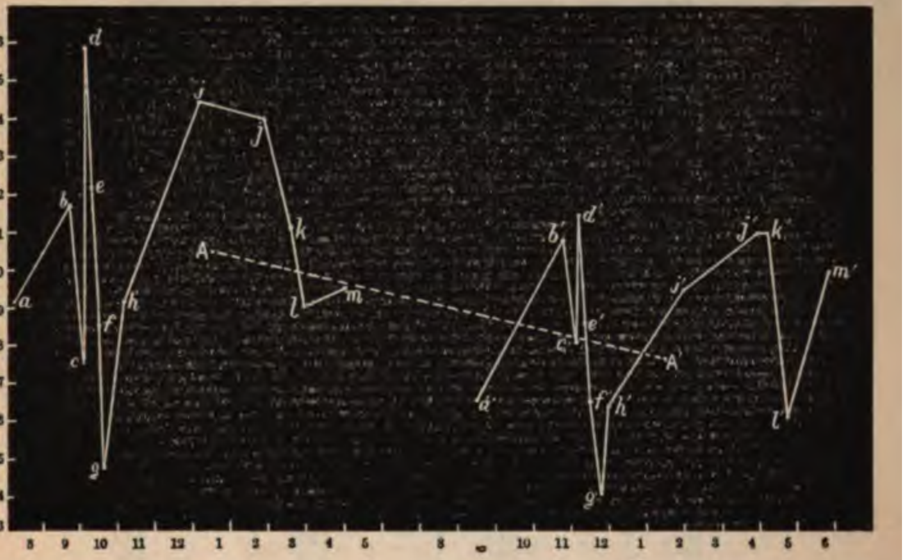
Before Walking.		After Walking.		
Time of Day.	Time in sec. for 300 taps.	Time of Day.	Time in sec. for 300 taps.	Difference.
8 a. m.	41.75	10 a. m.	43.75	2 sec.
10 a. m.	39	12 m.	43.5	4.5 "
12 m.	36.5	2 p. m.	44.5	8.5 "
2 p. m.	39	4 p. m.	44	5 "
4 p. m.	39	6 p. m.	46	7 "

This falling off in rate after a walk is probably due to (a) general fatigue and (b) mental relaxation induced by the walk, because a walk toward the close of the day when the normal rate was the highest, produced more marked effect than a similar walk in the morning.

Effect of Vigorous Mental Exercise. It was noticed by a comparison of records and the daily journal, that strong mental concentration, especially if accompanied with the excitement of interest, was favorable to rapidity of movement. To test this further, records were taken immediately before and after sorting into heaps as rapidly as possible 80 cards of 10 different kinds. Several packs were rapidly distributed with a few seconds rest between, until it required the utmost concentration to fix and keep the position of the different kinds in mind. The exercise is somewhat exciting and to make the best time it requires the most undivided attention. The following table gives the time in seconds required for making 300 taps before and after such experiments:

Before Sorting Cards.			After Sorting Cards.		
Date.	Time of Day.	Time in sec. for 300 taps.	Time of Day.	Time in sec. for 300 taps.	
Apr. 5	3.00 p. m.	44	4.00 p. m.	41	—3
" 8	10.45 a. m.	34.75	11.05 a. m.	38	+3¼
" 8	5.05 p. m.	39.5	5.45 p. m.	39	—½
" 9	8.10 a. m.	39	8.30 a. m.	36.5	—2½
" 9	11.10 a. m.	39.25	12.00 m.	36.5	—2¾
" 11	10.10 a. m.	46	10.45 a. m.	40.75	—5¼
" 11	3.40 p. m.	41	4.05 p. m.	41	0
" 12	4.00 p. m.	39	4.45 p. m.	36	—3
" 13	10.35 a. m.	38.5	11.20 a. m.	38.5	0
" 14	1.15 p. m.	44.5	2.00 p. m.	39.5	—5
" 15	9.45 a. m.	41.75	11.05 a. m.	41.5	—¼
" 16	10.05 a. m.	37.5	11.45 a. m.	34	—3½
" 18	10.25 a. m.	42.25	11.30 a. m.	36.5	—5¾

The similarity of the curves for these figures is very striking, and they also show the marked difference in rate.



I. Before Sorting Cards.

II. After Sorting Cards.

NOTE.—Points *a*, *b*, *c*, *d*, etc., curve I, represent tests before sorting cards, and *a'*, *b'*, *c'*, *d'*, etc., curve II, corresponding tests after sorting cards. The dotted line, *A A'*, connects the points representing the averages before and after sorting the cards.

After studying the variations caused by such severe mental work, and taking into the account that vigorous physical exercise had produced the opposite effect, it seemed probable that the increase in rate was due to increased central activity and the unconscious tension of the muscles attending this central excitation. This view was somewhat strengthened, when a study of the whole series of experiments revealed the fact that an increased rate had accompanied mental excitement. For example, the rate was increased after reading an interesting unexpected letter, after the announcement of a distinguished visitor to inspect the work, and just before reading a paper before the psychological seminary. While these observations are too few to base upon them definite statements, they are of value, because such conditions cannot be manufactured at will.

From the results of these observations the suggestion came that such influences might throw light on the difference between sensory and motor reaction-time. The following table shows a series of reactions before and after sorting cards as previously explained, given in thousandths of a second and obtained with a Hipp chronoscope:

Sensory.		Motor.	
Before Cards.	After Cards.	Before Cards.	After Cards.
128	161	117	124
131	123	103	131
215	134	116	121
131	146	111	116
130	151	116	117
197	151	119	115
165	148	123	117
147	114	104	107
181	129	118	110
141	140	115	109
126	161	126	103
157	143	142	105
129	107	158	118
116	123	122	108
133	137	130	109
136	115	140	115
154	131	136	121
138	110	232	104
134	117	149	114
214	126	108	129
139	164	117	107
121	191	115	111
151	132	130	146
130	136	131	98
171	140	156	119
160	129	121	116
155	200	123	126
140	166	107	113
215	135	126	113
180	121		
Aver. 152.1	Aver. 139.3	Aver. 128.3	Aver. 118.6
Av. var. 19.1	Av. var. 16.6	Av. var. 15.5	Av. var. 8.06

These differences in reaction-times—which are relative and not absolute—must depend chiefly and primarily on changed central conditions; for even if, as has been suggested, the muscles are in a greater state of tension after such mental exercises, it is a result of an unconscious partial innervation of the muscles of the body accompanying increased central activity. If it be true that general muscular tension is produced by mental concentration, it leads to the very important suggestion, viz.: that the chief cause of the feeling of bodily weariness resulting from mental concentration, may be due to muscular fatigue.

Experiments on Others. Experiments were made at my request by two other men. They followed the same programme that I had used. Records were taken for seventeen days by B., and for six days by L. In neither case was there developed any marked daily rhythm. B.'s averages for the different hours, show the greatest rapidity at 8 a. m. and the least at 6 p. m., while the rate at the other four hours was approximately the same. The mean variation is so large, however, that these averages are comparatively meaningless. The daily rhythm in my own case did not make its characteristic appearance until after almost a month's practice. The only definite results from the work of B. and L. are their rates and the effect of exercise which will be spoken of further on. B.'s rate for the whole series of tests made was 6.4 taps per second; for L. the rate was 6.8 taps per second.

During the course of the experiments tests were made upon a number of visitors. The accompanying table shows their rates, and is of interest in that great individual differences are shown.

RATE OF VISITORS.

Date.	Individual.	Time of Day.	Time in sec. for 300 taps.		Date.	Individual.	Time of Day.	Time in sec. for 300 taps.	
			Right Hand.	Left Hand.				Right Hand.	Left Hand.
1892 Feb. 5	A.	11.10 a. m.	45	68	1892 Mar. 8	L.	2.30 p. m.	47	
" "	B.	11.30 a. m.	46	49	" 12	M.	5.30 p. m.	57	
" "	C.	11.00 a. m.	42.5	51.5	" 12	N.	5.30 p. m.	47.25	
" "	D.	6.00 p. m.	46	50	Apr. 4	O.	10.00 a. m.	53	
" "	E.	9.00 a. m.	44	57	" 10	P.	12.00 m.	50	
" 9	F.	10.30 a. m.	45	60	" 10	Q.	1.00 p. m.	67	
" 9	G.	4.00 p. m.	46	54	" 10	R.	1.00 p. m.	51.5	62
" 9	H.	4.10 p. m.	49		" 15	S.	2.00 p. m.	66	
" 9	I.	4.15 p. m.	51.5		" 15	T.	2.00 p. m.	57	
" 22	J.	4.30 p. m.	52						
Mar. 4	K.	4.00 p. m.	48						

Effect of Practice. The average rates of B. and L. were reached on the third day, and no perceptible increase came in the remainder of two weeks' work. The increased normal rate seemed to be chiefly due to the fact that fatigue ceased, which was shown in the greater regularity of the curves. All visitors who took a record for the first time complained of fatigue, and the change in the direction of their curve toward the close of a record plainly showed their diminishing rate. In my own case the average ability had been attained while testing and perfecting the apparatus previous to recording any work. In order to test the effect of exercise of the right hand upon the left, the rate of the left was found at the beginning of the experiment, and again, after more than two months of work. The result shows no gain in the left. If the rate of the left had been influenced any it occurred before any records were taken.

Effect of Rest. The effect of a day of rest, when the subject was in good health, was very slight as shown by comparing the records of two Saturdays and two following Mondays:

Saturdays.							Mondays.						
Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.	Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.
Time in sec for 300 taps.	43	41	36	40	36	40	Time in sec. for 300 taps.	42	43	38.5	43	40	43.25
	39	37.5	36.5	40	36	38		40.5	40	39	40	36	42

From the above table it is seen that the control was slightly weakened for Mondays rather than strengthened; a marked difference is shown between two Saturdays and the following Mondays, during a period of overwork, resulting in a week's illness:

Saturdays.							Mondays.						
Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.	Time of Day.	8 A.M.	10 A.M.	12 M.	2 P.M.	4 P.M.	6 P.M.
Time 'n sec. for 300 taps.	37	38.5	37.5	39.5	34.5	37	Time in sec. for 300 taps.	38.75	32.5	32.6	30	30.5	32
	37	35	35	36.5	34.5	Ill		40.5	36	38.5	33	Ill	Ill

These observations are recorded merely from their suggestiveness.

Although we have subjective evidence of varying degrees of mental activity we have very few methods of accurately estimating its condition. The fact that central activity is accompanied with the power to make rapid voluntary movements suggests that the rate at which voluntary movements can be made, may be taken as something of an index to central nervous activity. The following facts may be stated as the chief results of this research:

1. The apparatus described furnishes a convenient and accurate method of recording and counting rapid voluntary movements.
2. The normal rate for the most rapid voluntary movements of the right wrist was found to average 8.5 taps per second.
3. During the work a daily rhythm was developed with the slowest rate at 8 a. m., and the most rapid at 4 p. m.
4. A vigorous walk decreases the rate for rapid voluntary movements of the wrist.
5. Increased central activity favors increased rapidity for voluntary movements.

EXPERIMENTAL RESEARCH UPON THE PHENOMENA OF ATTENTION.

BY JAMES R. ANGELL AND ARTHUR H. PIERCE.

These experiments have been carried on in the Harvard Laboratory with the purpose of discovering the correct interpretation of some curious results first obtained by Prof. Wundt in the study of attention. Prof. James and Prof. Wundt differ in their explanation of the facts and we have attempted to ascertain which, if either, was right. We have come to a conclusion differing from both, which we venture to offer with some assurance that our results will bear the test of verification by other observers. At the outset we shall state briefly the conditions of the experiments as conducted by Wundt and the explanations adduced by him and by Prof. James.

The essential question is whether we can interpret *as simultaneous* two or more disparate simultaneous sensations, and if not, how to explain our errors. For example, as Wundt conducted the experiment, the observer is seated before a graduated dial over which moves a pointer like the minute hand of a clock. The hand does not make continuous revolutions, but vibrates back and forth, each vibration traversing a trifle less than a circumference. A mechanical device enables a bell to be rung at adjustable positions of the pointer, and the subject is required to fix the location of the hand when the sound is heard. The experiment can be complicated, as was done by von Tschisch, through adding electric and tactile stimuli, but his results from these variations show no essentially new factors and are easily explicable under our hypothesis. Furthermore the fundamental conditions are all present in the simpler form, so we shall hereafter deal with that alone. Now, Wundt, found three classes of results. First, *correct results*, when the sound was heard at the position of the pointer where it actually occurred. Second, *positive displacements*, when the sound was heard at a position of the pointer after it had passed the real place. Third, *negative displacements*, when the sound was heard before the pointer had come to the real place. When the revolutions occurred once in a second he found the errors least frequent. When the rate was faster positive errors predominated, when slower, negative. Wundt has very properly emphasized the peculiarities of different individuals and the differences in the same individual at different times. In this we heartily agree with him and it is no part of our purpose to question any results he has attained. But his explanation strikes us as faulty. He would account for all the variations by the peculiar laws of the ripening of apperception. Thus he assumes that the apperception keeps periodically ripening after each stroke of the bell in anticipation of the next stroke. It may ripen more slowly or more rapidly than the occurrence of the strokes. If faster, then the sound is heard too soon. If slower, it is heard too late. In any event the position of the index at which it is heard is identified as the correct one, and this is obviously determined by the rate of ripening peculiar to the individual. A word should be said at this point in explanation of Wundt's actual procedure in an experiment. He always finds a single revolution of the pointer insufficient to locate the position at which the sound occurs. The motion must continue for a sufficient length of time to allow the sounds to form a regular series. A certain region of the dial is then perceived as the general location, and individual points in this region are then selected until the mind is satisfied.[1]

The general nature of Prof. James's criticism will appear from the following quotation from his comments on this subject (Prin. Psych. page 415, Vol. I.): "The bell or other signal gives a momentary sensation, the index a continuous one, of motion. To note any one *position* of the latter is to interrupt this sensation of motion and substitute an entirely different percept—one, namely, of position—for it during a time however brief. This involves a sudden change in the manner of attending to the revolutions of the index; which change *ought* to take place neither sooner nor later than the momentary impression, and *fix* the index as it is then and there visible. Now this is not a case of simply getting two sensations at once and so feeling them—which would be an harmonious act; but of *stopping one* and changing it into another (i. e. exchanging one for another) whilst we simultaneously get a third. Two of these acts are discrepant, and the whole three rather interfere with each other. It becomes hard to 'fix' the index at the very instant that we catch the momentary impression; so we fall into a way of fixing it either at the last possible moment before, or at the first possible moment after, the impression comes. This at least seems to me the more probable state of affairs. If we fix the index before the impression really comes, that means that we perceive it too late. But why do we fix it *before* when the impressions come slow and simple, and *after* when they come rapid and complex? And why under certain conditions is there no displacement at all? The answer which suggests itself is that when there is just enough leisure between the impressions for the attention to adapt itself comfortably both to them and the index (one second in Wundt's experiments), it carries on the two processes at once; when the leisure is excessive, the attention, following its own laws of ripening, and being *ready* to note the index before the other impression comes, notes it *then*, since that is the moment of easiest action, whilst the impression, which comes a moment later, interferes with noting it again; and finally, that when the leisure is insufficient, the momentary impressions, being the more fixed data, are attended to first and the index is fixed a little later on. The noting of the index at too early a moment would be the noting of a real fact, with its analogue in many other rhythmical experiences. . . . . . . Wundt's explanation (if I understand it) of the experiments requires us to believe that an observer . . . . shall steadily and without exception get an hallucination of a bell-stroke before the latter occurs, and *not hear the real bell-stroke afterwards*. I doubt whether this is possible, and I can think of no analogue to it in the rest of our experience."

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[1] For a fuller account of the matter see Physiol. Psych. 2nd ed. II 264-6, 273-4; 3rd ed. II. 339; Philos. Studien, II. 601 ff. Also Pr James's Psychol. I. 410 ff.

It is no part of our purpose to follow out the differences of opinion merely as such which exist between Professors Wundt and James concerning this matter. We trust the above statement has made their respective positions clear while bringing out the exact nature of the experiment and the points at issue. We believe that there is truth in the explanations of both, but our criticism is that neither at all suffices to exhaust the problem, which proves to be much more complicated than one could possibly suppose from the mere reading of an account. Wundt has justly remarked that the research requires years of experimentation before adequate conclusions can be reached. And yet this is not wholly true. Wundt has worked many years on the subject himself, but his main interest has apparently been to obtain a sufficiently large number of results to justify comprehensive and exhaustive inductions. So far so good. We are, however, unable to see that careful experimentation covering a much shorter period is necessarily imcompetent to isolate with considerable accuracy the essential psychological processes involved. We think Herr Wundt has overlooked some very important factors and it is on this account that we venture to offer our conclusions. By no means would we on any other ground oppose our experiments, which are only numbered by hundreds, to his, which must mount into thousands.

In front of the dial of our apparatus revolved a pointer, carrying at the back a wire, bent so as to sweep through a movable cup of mercury at the rear of the dial, thus completing an electric circuit which passed through a telegraph sounder, the latter giving a loud click whenever the circuit was closed.

Various reasons led us to abandon Wundt's machine. The specimen at our disposal was faultily constructed in several particulars and accurate experimentation with it seemed almost impossible. In addition to this, the noise made by the clock work, which was the motor power, was very distracting to the observer and we felt sure it must vitiate the results. The principle of the pendulum seems to us unfortunate in this experiment, because the pointer starts from perfect quiet, moves rapidly faster and then slows up to quiet again, thus introducing a constantly changing rate of motion. Most objectionable of all was the bell. The vibrations of course last some time, and the sound of the actual stroke becomes so blended with this after-tone that the difficulties of identifying the stroke with any one position of the pointer are vastly increased. An attempt was therefore made to construct a machine which should possess the following qualifications: first, lack of distracting noises; second, uniformity and steadiness of motion in the rotating pointer; third, an incoming sound which should be short and flat.

The motor power which appeared most convenient and reliable was an ordinary kymograph. To be sure, this is not a noiseless instrument, owing to the whir of the governor, but the noise is by no means of the nature of a disturbing click and we have had no complaint of disturbance from this source on the part of any of our subjects. Of course the ideal motor would be noiseless. The motion was transmitted from the drum of the kymograph to the pointer-shaft by means of a belt of $\frac{3}{4}$-inch webbing. As an upright to support the pointer a Bradley color-wheel was utilized, the pointer being set on the shaft in the place usually occupied by the color disc. The pointer, made of thin, light wood, gouged on the back to reduce the weight as much as possible, was 63 cm. in length, the tapering end having a length of 47 cm. from the center of the shaft. Pieces of sheet-copper were fastened to the upper end to secure an accurate balance and tests were made occasionally during the observations to ascertain if the equilibrium was perfect.

A dial was made of heavy pasteboard held upright by small wooden supports at each end, which were sunk into a strip of wood; as shown in

FIG. 5.

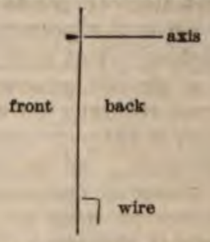
the accompanying figure. The graduation of the dial and t
ing should be as uniform as possible in order that no one

present any greater interest than another. The slightest thing is sufficient to divert attention and so vitiate results.

As the figure shows, the dial was only a portion of a circumference. The inner edge of the dial was 43 cm. from the center, so the *entire* inner circumference would have been 270.18 cm. in length. The available portion of the scale was 47 cm. long—a little more than $\frac{1}{6}$ the entire circumference. This, however, was deemed long enough, as it gave considerable range to vision without requiring any rotation of the head and much less of the eye-balls than would have been necessary with a larger portion of the circumference.

To secure the short, flat sound so desirable we employed an electric telegraph sounder. At the back of the dial and separated from it by the width of its wooden supports was fastened a brass rod of about 3 mm. diameter and of the same curvature as the dial itself. On this ran a carriage bearing a cup of mercury. Down the back of the pointer from the shaft on which it rotated ran a wire which, sweeping through the mercury, completed the circuit passing through the key. The carriage was constructed as follows: upon a small wooden block through which the curved brass rod passed was fastened a small wire-coupler or binding screw. To the head of the screw a bit of glass tubing 6 mm. bore was secured by means of sealing wax. This tube contained the mercury. A small brass wire from the metallic part of the carriage was wound several times around the rod on which the carriage traveled, thus making an electric connection between the rod and mercury. The tension of the wood upon the rod was sufficient to hold the carriage steadily in whatever position it was put, and at the same time not enough to prevent an easy movement from place to place. The whole of the metallic portion of the carriage could be rotated slightly in a plane parallel to the dial and the mercury cup could be raised or lowered by the screw on which it was fastened, thus rendering any necessary adjustment easy and rapid. The carriage, with all that pertained to it, was wholly concealed from the view of the observer, whose eyes were on a level with the dial.

The light brass wire running down the back of the pointer was bent near the top of the latter, as the accompanying diagram shows.



So that while the pointer revolved in front of the dial-scale the end of the wire swept through the meniscus of the mercury behind the dial. A Leclanché battery gave entire satisfaction throughout. The circuit was as follows: From one pole of the battery through the curved brass rod to the mercury; thence, when the pointer completed the circuit by sweeping the mercury, up the pointer to the metal shaft, on which rested a platinum wire connected with the key, and so back to the other pole of the battery. Whenever the circuit was closed the key gave a sharp click.

The dial was divided into spaces of about a centimeter in length, numbered from left to right. The outer gauge of the kymograph and the internal changes of gearing made a large number of rates possible, but we selected certain ones for the sake of uniformity to represent slow, medium and rapid movement. The time of rotation at these various rates was carefully ascertained by means of a metronome with the following results:

Slow rate—One revolution in 6.06 seconds. Outer gauge at 15. Lower wheel of kymograph in gear, with upper movable wheel in its middle position.

Medium rate—One revolution in 3.6 seconds. Same gearing as above with gauge at 30.

Fast rate—One revolution in 1.2 seconds. Lower wheel out of gear, and upper wheel at extreme left, i. e., toward inner wall of case. Gauge at 30.

The time occupied by the pointer in crossing a single space was accordingly .0219 sec., .0152 sec., .0043 sec., respectively for the different rates.

With these preliminary considerations we are now ready to consider our method of procedure and results. We may say at once that we meet the same facts as Wundt, i. e., correct results, positive errors and negative errors, though the ratio existing between the two sorts of errors does not correspond, nor yet the conditions under which they are found appearing. This, however, does not alter the fact that we have identical cases to explain. A word or two describing the modus operandi of a typical experiment will serve to clear up future explanation. Our usage has been this—to follow the pointer on its first revolution until the sound is heard, when we attempt to stop the movements of the eyes instantaneously. The point on the dial thus attained is made the basis of observation for the next revolution when any seemingly needed correction is made; to the right, if the sound seems to occur after the pointer has passed the given point; to the left, if before. But now in this very process of correction enters in a *double* process which neither of us observed at the outset, though it was later seen to play a great part. Fortunately it subjects itself with some readiness to independent experiment. What seems at first a bare attempt to get a coincidence of the sound with a certain position of the pointer proves on more acute observation to be a vibratory process in which attention is *primarily* directed in alteration now to the visual factor and now to the auditory. Thus our natural method seemed to be to pick out an approximate point on the dial and then watch for its obliteration by the revolving pointer. Immediately it was obliterated, the attention was turned to the sound to determine whether it had occurred or not. By repeating this process and making needed corrections, a point was attained on
dial which seemed to minimize the interval between the
and the sight of the pointer obliterating the spot.

"minimize" advisedly, because we early discovered how easily one can be deceived into believing a point quite remote from the correct one to be the right one. We could not discover that there was any greater feeling of surety when a correct result was obtained than when an erroneous one. The two sensations *seem* to be essentially simultaneous and yet there is seldom any feeling of security. But in the method just described the obvious attempt was to make the visual element fundamental and then to hitch on the auditory. We were trying to attend to the sound *after* we had gotten a certain sensation of sight. Further introspection showed that however genuine was our attempt thus to exalt the visual factor, the truth really was that attention vibrated and sometimes the sight, sometimes the sound, was made fundamental. If we may be allowed a homely metaphor, now one element, now the other, served as hitching post. Before going on to discuss the varying results we may mention a fact which seemed to modify the above method in a measure. One of us noticed a strong sense of rhythm from the sounds. This was observed expressing itself in more or less unconscious muscular contractions and movements, such as the nodding of the head, the beating of the fingers, etc. This naturally suggests Wundt's ripening of apperception. What part this plays in the results will be remarked later on. Though we lay no stress on this portion of our work, it may be fair to state that the readings which we have obtained from first revolutions show almost without exception positive errors. The first twenty-five final readings of each new subject generally show a predominance of negative errors and then the positive errors come rapidly to the front. So that differing from Wundt we find a preponderance of positive errors. Furthermore we have been unable to detect any constant influence upon the character of the errors due to alterations of speed. We have made this test on the Wundt machine as well as our own, using the extremes of speed.

Now let us turn our attention to the explanation of positive errors in which the sound is identified with a position of the pointer beyond the place where the sound has occurred. This is the error which common sense would assume as natural. In the cases where we obtain a reading from the first revolution of the pointer, the positive error is easily explained as a dragging of the eyes by the moving pointer. It amounts here to a simple reactive experiment. The positive error is explained by the time consumed in transforming the incoming auditory sensation into a motor act, which is in this case the stopping of the eyes. When the eyes have actually stopped, time has therefore elapsed and the pointer having passed on is thus read too far to the right. The positive error made after deliberation and also after long practice requires further explanation. If we represent the auditory and visual elements by their initial letters, we may express the vibrations which we find occurring in this way—either V—A—V or simply A—V. In the first case attention is directed to the dial, watching for the obliteration of the selected spot by the pointer. Suddenly it is shifted to catch the sound, and this occurring at once, or a moment later, the attention is shifted back again to the pointer, which is now naturally a trifle beyond the actual point of connection. In the second case, A—V, the first step in the above process is omitted. Indeed, it may be disregarded in any event, as the last vibration is the one affecting the result and giving a positive error. When the great ease with which attention fatigues is remembered, the basis of this explanation will be seen to have great strength, and actual experiment will add to its power. To prove the matter conclusively we made a number of experiments in which the sound was purposely made the primary element, to which we then tried to attach the sight of a given position of the pointer, that is, a position selected by the experimenter as approximate. All the errors under these conditions were positive and many of them very large.[1] Such an arrangement makes the experiment essentially one of reaction, though an apperceptive reaction, and the positive error is amply provided for under the necessity of a certain lapse of time requisite for the sound to be registered in the brain and then identified as simultaneous with a given position of the pointer. It may be objected that this is an essential alteration of the real experiment. To this we in a manner assent, simply insisting that the experiment is of such nature as to assume of itself just this (among others) unexpected form. What we have done is by no means to arrange artificial variations, but simply to observe the metamorphoses undergone by the experiment itself. It was not without reason that we remarked at the outset the exceeding complexity of the problem. Occasional positive errors, like occasional negative errors, are doubtless to be explained as due to the arbitrary and inexplicable fascination of the attention by some one spot on the dial which the observer then tends strongly to regard as the correct one. These freaks of attention, based on the unaccountable interest felt in infinitesimal differences of objects, must be assumed as constant. The positive errors, then, attained from readings of first revolutions are explicable on the basis of time consumed in mere reaction. Those occurring in later readings are also due to the lapse of time involved in an apperceptive reaction. In short, attention vibrates, and this vibration may be of sufficient strength to overpower the factors at work in negative errors and so by the time consumed in the process produce positive errors.

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[1] Confirmatory of this are the results of von Tschisch, who found the errors becoming positive when other interrupting sensations—as tactile and electric—were added to sound. The number and disparate quality of the interrupting sensations possessed strong interest, and the form of vibration, represented as A—V, occurred only after the attention had dwelt for a time on the combined sensations here grouped under A. Furthermore, the greater inherent interest of this com [illegible] as compared with the dial and pointer, would tend to [illegible] in anticipation of these combined sensations and so [illegible] as von Tschisch did, a preponderance of results in [illegible] vibration occurs, thus producing positive errors.

We must next consider negative errors. It will be recalled that we mentioned the fact that in our experiments negative errors predominated during the first twenty-five results, and from that time on positive errors constituted a large majority. In one case the first twenty-five results were all negative errors, while in the ensuing hundred and twenty-five only three negative errors occurred. We may state at once that we consider the factor ordinarily most efficacious in producing negative results to be unconscious, or involuntary, over-correction, which we shall proceed to elucidate. Experience teaches mankind that in the case of moving objects, similar to the pointer, the attempt to stop the eyes from following has generally resulted in their being carried along a little beyond the point desired, and we have become accustomed to rectifying this mistake. What was once a conscious effort has now through psychic education become practically automatic. Instances of education of this general type are common enough. Learning to shoot on the wing, catching or batting a ball are excellent illustrations. But it is still competent for some one to object that this does not explain enough. It may explain why, after the eye has left the pointer, it should swing back to the left, but not why there should even then necessarily be a negative error. Why does not the eye stop at the right of the correct point and so give a positive error? To this we reply that the special rate of motion manifested by the pointer introduces an essentially new psychic, as well as physiological, factor. What we mean is that *rates* of motion as we experience them in life are so exceedingly various as to render accurate acquaintance with any of the faster forms, such as we deal with in this experiment, quite improbable. Thus the subject, when first experimented upon, is really encountering a unique set of experiences to which he has not yet learned to accommodate himself. We think that what happens physiologically (and so explaining the negative error) is this: there occurs a too complete relaxation of the ocular muscles involved in making the eye follow the pointer, coupled with a spasmodic—and thus too severe—contraction of the reversing muscles. It is in truth an illustration of the difficulties encountered in the accommodation of an organism to a new environment. The case is perfectly analogous to the attempt to catch a falling goblet or vase, where one almost invariably spreads disaster through the spasmodic nature of his effort. Again it may be objected that while our explanation so far may hold good for the cases of first revolutions when the eyes are obviously following the pointer, it is considered fallacious as accounting for negative errors in later cases conducted as we have described, i. e., by watching for the obliteration of a given point by the pointer. The objection has only slight importance, because, no matter how intently the gaze be fixed on the dial, it is practically impossible to prevent a slight vibratory movement of the eyes when the pointer passes, and the spasmodic motion is only reduced in power, but by no means eliminated. In any event this is not the whole story about negative errors. We are, however, convinced that this unconscious over-correction plays a very large part in the production of these errors. In terms of our vibration formula this process would be expressed as V—A—V + unconscious over-correction.

But the negative error may be produced in another way. Suppose, on a dial graduated and numbered like ours from 1 to 12, the space between 5 and 7 had been settled on by the observer as the general region in which the sound occurred. As the pointer comes round and gets to 5, the attention, which has just previously been concentrated on the visual factor, is instantly shifted to the auditory, and if that occurs in quick sequence the mind feels satisfied that the two have occurred simultaneously. Attention is in this case not shifted back to the visual element. The vibration is expressed as V—A. Apperception time doubtless plays a part here again, but while it enters in to cause error, it has of itself no especial preference for either kind of error so far as we know. A variation which we have next to notice may be wrongly charged to apperception, but we repeat, we know of no facts tending to show *with any conclusiveness* that the apperception time of sound is either longer or shorter than that of sight.

On the other hand there is experimental ground for believing that the photo-chemical process in the retina consumes an appreciably longer time than the vibratory process
cochlea, so that sight is a slower affair than hearing
gives the ear an advantage of .049 sec. Now at the

rate employed by us the pointer traversed one space in .0219 sec. Therefore the .049 sec., by which the eye is slower than the ear, is equivalent to more than two spaces traversed by the pointer. We are inclined to regard these particular figures as provisional and subject to alteration under more extended experimentation, but it may be accepted as proven that sight requires longer than hearing and enough longer to play no inconsiderable part in this experiment. Obviously the effect of this fact would be to cause negative errors, for, to take an example, when the pointer was *seen* at 6½ (6 and two spaces) it would really be at 7, and if the correction was made at 7 the sound would be identified with the position 6½ rather than 7, though the time consumed in transmitting the auditory sensation would throw the reading slightly forward toward 7. But it is clear that the effect of this condition of things must be overpowered occasionally by the processes mentioned in our discussion of positive errors, otherwise we should continually get negative results. Aside from our experiments, which point strongly to the fact, there is good *a priori* ground for supposing that experience teaches us to make correcting judgments to compensate for these discrepancies among our senses, so that the gradual disappearance of negative errors under practice is what we should expect, Wundt's results to the contrary notwithstanding. It should be understood that this transition to positive from negative errors occurs without the subject being informed of the nature of his errors, indeed without his knowing whether he is making any errors at all. It seems to be a result of finer powers of discrimination and attention resulting merely from practice. It seems highly probable, then, that when a subject has become practised in the experiment, the part played by unconscious correction of the oculo-motor effects and the tardiness of the photo-chemical processes gradually dwindles into insignificance, and negative errors, when they occur, which is rare with us, are rather due to the vibratory process of attention which we have indicated by V—A. Again we subjected our theory to separate experiment by consciously trying to make the visual factor primary and the auditory secondary without a return to the visual. The result substantiated our hypothesis. In a subject whose total results show a ratio of 3 : 1 in favor of positive errors, this procedure altered the ratio to 4 : 3 in favor of the negative errors. The experimentation was not sufficiently extended in this direction to warrant our ascribing any final validity to this test. It served to satisfy us quite conclusively, however, because the actuality of the process supposed showed itself clearly.

We mentioned earlier in the paper the observation of a feeling of rhythm gained from the succession of sounds and expressing itself in various muscular contractions. We have now to consider the effects of this. It will be remembered that Wundt's explanation hinges on the ripening of apperception. The *natural* rate of ripening may be faster, or slower than the rate of occurrence of the sounds, or identical with it. The different results found their explanation in this fact. Prof. James's explanation finds a somewhat similar basis. The rhythmic feeling we mention should have a very similar effect. With our machine, which gives its sound less frequently than Wundt's, we might naturally suppose that the impatience to "let off" the muscular contractions would produce a tendency to negative error. But there is nothing to substantiate any such sweeping assumption. The peculiarities of the individual may cause this to affect the result in either direction, and by most observers the feeling is not noticed at all. In short, it is another factor which we must consider constant in its influence.

What shall be said of the correct results? It would seem to be sufficient reason for assuming these to be accidental that one has no greater feeling of surety when giving a correct reading than when giving an erroneous one. It is by no means impossible that the different factors which enter into the experiment should occasionally so combine as to produce correct results. Similarly we see no reason for supposing that these same factors may not at any time so combine as to produce either kind of error. We admit the inherent charm of such accounts as those of Prof. Wundt and Prof. James, which seem to involve only a single general principle, but we cannot feel that these at all cover the ground. In any case we do not think the present kinds of apparatus at all competent to decide dogmatically whether the mind does ever succeed in detecting exact simultaneity in such cases aside from mere chance. Our machine is much more accurate in this respect than Wundt's, but it is exceedingly difficult to be sure that a given position of the index is *absolutely* and *exactly* simultaneous with the click. For our own part, we do not believe the mind ever does feel two such stimuli as exactly simultaneous in any other sense than that in which it feels rapily successive and disparate stimuli to be simultaneous. Excessively rapid vibration it is capable of and this answers its purpose. But apparently it has no criterion by which to distinguish exact similarity of disparate stimulations from rapid sequence of the same.

But now a word about Wundt's method of conduct
task which he set himself. The problem which he

he states thus: to examine what shall happen "when we receive a *series* of impressions *separated by a distinct interval* into the midst of which a heterogeneous impression is suddenly brought." But in the method adopted does Wundt really accomplish his avowed purpose? Does the moving of an index hand over a scale really give a *series of impressions separated by a distinct interval?* Not only is this not strictly speaking the case even if one succeeds in following the index hand round the dial, but most of all it is not the case when one adopts the method of fixating a point on the scale for trial reference, repeating the process until satisfaction is gained. Wundt apparently followed the former method, but even if the attempt is thus made to follow the pointer constantly, one soon discovers that he is dealing with no series of impressions separated by distinct intervals. Some portions of the scale will be seen with much greater clearness than others. The constantly changing rate of motion in the hand of Wundt's machine, owing to the pendulum principle employed, adds greatly to this defect inherent in the observation of any body moving across a graduated scale. To obviate this difficulty we attempted to construct an apparatus by means of which a *genuine series of impressions separated by distinct intervals* should be attained. We succeeded in producing a device by which the letters of a series were presented to the eye one at a time. The sound was made to coincide with one of the letters. Two forms of the apparatus were employed, the differences between them appearing from the following description:

FIRST FORM.—Nearly the same materials were used as in the machine previously described. The scale and pointer were removed to the rear of the support, the pointer being attached to the other end of the shaft. Both scale and pointer served simply to maintain the old method of electric connection. At the front, in the place formerly occupied by the pointer, was placed a circular piece of paste-board, 40.5 cm. in diameter, at the circumference of which were placed the letters of the alphabet arranged at regular intervals. The letters were from a set of Dennison's alphabets, size 31. The circle bearing the letters was made to revolve as before by means of the kymograph. About six inches in front of the revolving circle was placed a wooden box large enough to admit the head with ease and not too deep to offer inconvenience to the observer. In the back of this was cut a window 1 cm. square and exactly on a level with the position of the lowest letter on the circle. The box was blackened on the inside to prevent any distraction to the eyes. The position of the box and its distance from the circle could of course be adjusted to the observer. The circle being fitted on to the shaft like a color-disc, it was only necessary to rotate it on its axis to secure the exposure of any letter and then fasten ening the screw which held it in position.

SECOND FORM.—In the form just described the observations were necessarily made with the use of only one eye. The speedy fatigue under these conditions led us to alter the form as follows: In place of the revolving circle bearing letters to be viewed through a stationary window, we utilized a circle of black paste-board slightly larger than the last and like it revolving on the shaft. Near its edge a small window was cut through which a series of stationary letters behind could be viewed as the window exposed them one by one. We used a smaller set of letters for this, No. 21. They were arranged upon the arc of a circle whose radius was equal to the distance from the center of the shaft to the lower edge of the window in the paste-board circle. They were separated from each other by spaces of about 1 cm. The adjustments could be made as before. To avoid excessive rotation of the head and eyes, we arranged so that only about ½ of a circumference was exposed for experiment and this on a level with the observer's eyes.

These variations in form have had two points of value for our conclusions. In the first place we find that when you have a genuine series of visual factors regularly succeeding one another your results become disturbed by the effects of after-images. The consequence is an increase in negative errors. For example, suppose in our experiments the electric contact was made at the letter C. It frequently happened that the after image of B or A (which had gone before) would come out with sufficient vividness to be identified with the sound and the attention, being then *set* for A or B on the next revolution, would tend to settle on one of them as the correct letter. The occasional vividness of these after images was very marked and we can now see that it may well play some part in the original form of the experiment. The second consideration is this: that conducting the experiment as Wundt proposes with a series of impressions separated by regular intervals into which is introduced a heterogeneous impression, there is no appreciable effect upon the result other than the effect just noticed of allowing after images to play conspicuous part. This is an additional reason in our minds for distrusting any explanation which regards the facts as capable of arrangement under any one general law, such as the ripening of apperception. The factors causing the different results are far too heterogeneous to admit any such arrangement and, so far as we can see, they *may* at *any time* combine in such relation as to produce either kind of result.

In conclusion, allow us to repeat that we by no means regard our results as warranting any very comprehensive inductions. We question the possibility of making such for many years to come. We hope to have done some service in bringing out a few of the peculiarities in the phenomena of attention and to have suggested in a measure the unexhausted richness of a very simple experiment in revealing these. We take great pleasure in acknowledging our obligations to Prof. James and Dr. Herbert Nichols for much valuable advice and suggestion.

SOME EFFECTS OF CONTRAST.

By A. Kirschmann, Ph. D., of Leipzig.

1. *Kinds of Simultaneous Contrast.* The forms of simultaneous contrast in the domain of sight can be reduced to the following:

1. Contrast in brightness.
2. Contrast in saturation.
3. Contrast in color.
4. Contrast in emotional tone.

The contrast in the size of the surface is omitted as belonging to all the space senses.

These kinds of contrast depend on certain conditions. The strength of the brightness-, color- and saturation-contrasts depends on the intensity of the light emitted from the object; the strength of the color- and saturation-contrasts is dependent on the shade of color and the degree of saturation of the contrasted surfaces; the strength of the contrast is also dependent on the extent of the contrasting surfaces and on the distance of the objects from one another.

The relations of the various kinds of contrast to these variables I have investigated. The results which have been published elsewhere[1] can be summarized as follows:

The intensity of pure simultaneous brightness-contrast, and probably also of the pure simultaneous color-contrast, increases, within the limits of clear perception of size in the resting eye, proportional to the linear extent of the inducing part of the retina, or proportional to the square root of the surface content. An intensity that produces a contrast can be replaced by a less intensity of a correspondingly larger extent without changing the strength of the contrast; that is to say, a relation of reciprocity obtains between the extent and the intensity of the colors that enter into the contrast. The simultaneous color contrast appears best when the brightness-contrast is excluded or reduced to a minimum. The simultaneous contrast between a color impression and a gray of equal brightness increases with the saturation of the inducing color. The simultaneous contrast between two colors is composed of two factors, the quantitative relations of which with a regular increase or decrease of the saturation of the colors change in irregular manner and in opposite directions. The mutual contrast between two colors reaches its maximum with a combination of medium degrees of saturation of the colors.

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[1] Kirschmann. *Ueber die quantitativen Verhältnisse des simultanen Helligkeits- und Farben-Contrastes*, Inaug.-Diss., Leipzig, 1890; also in Wundt's Philos. Studien, 1891 VI. 417.

2. *Black and White.* Among the effects of contrast there is one to which attention has not been called, although a neglect of the influence of contrast has led to a fatal error in Hering's theory of the fundamental colors. As is well known, his theory asserts the production of the various colors by the greater or less preponderance of the associative or the dissociative processes between two opposed pairs of fundamental colors. Likewise black and white are two such fundamental sensations and our different sensations of brightness without color are produced according to the preponderance of the one or the other of the processes in this pair. This assertation that black and white are elementary sensations is unjustified, as I shall attempt to show.

Right here, however, I must call attention to the fact that I do not have anything to say against the sensations of brightness, or the achromatic series of sensations of light, as such; this is, of course, too amply proved to need mention. But I do object to the unproved assumption that this series is produced by two antagonistic sensations just as a simple color is produced. On the other hand I am quite ready to agree to the view that black and white are extremes of a series which is not analogous to a series of simple colors, but to the degrees of saturation of a color; that is, white and black are not the extremes in the same way as red and blue-green are the extremes of the series of colors from red to blue-green, but are extremes in the same way that colorless light and the highest degree of saturation are the extremes of a series of any color in regard to its saturation.

Hering's theory regards colors as lying in two bipolar systems which have a point of indifference. If, e. g., the sensation red corresponds to the condition of assimilation and the sensation of blue-green to the condition of dissimilation of that particular substance, then colorlessness denotes the condition where the activity of the substance is zero. If we apply these relations to the black-white series, then the point of indifference is to be sought in the middle gray; to regard the middle gray as the zero-point of the activity of the black-white substance is a hypothesis that is not to be accepted lightly.

Are white and black really sensations? Hering's theory requires an affirmative answer to this; nevertheless the facts require, as I intend to show, a negative one. A phenomenon is to be considered a sensation, i. e., a simple mental phenomenon, when it cannot be further analyzed. It must maintain its character qualitatively unchanged even when isolated from all other mental elements. For example, the sensation red is not necessarily bound to any other phenomenon of consciousness. We always recognize this particular quality of light as red, no matter what the surrounding conditions may be, provided there is nothing abnormal about our organs of sight. In order to recognize an object as red nothing more is necessary than that we see it; isolate it from surrounding objects or bring it into any relation to them, it never appears blue or violet, but always red. Surrounding objects, however, do have a great influence on the degree of saturation; the shade of a given red will be different accord-

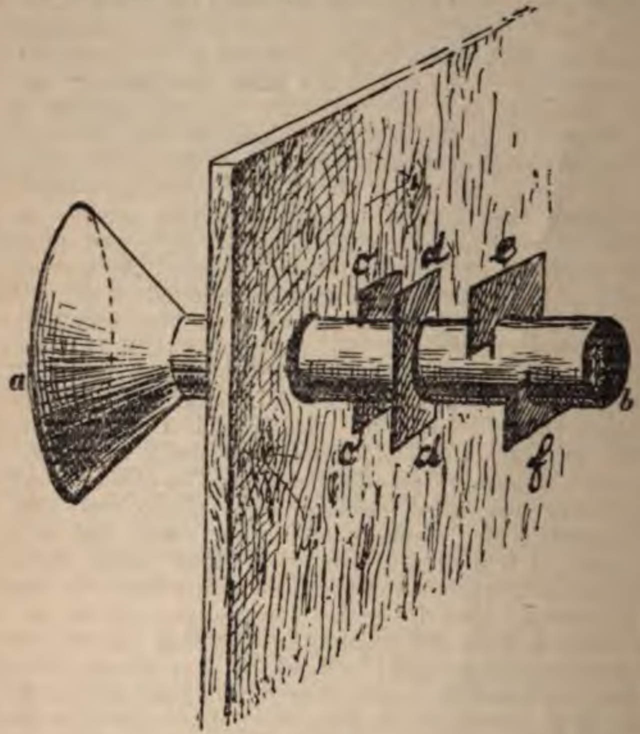

FIG. 6.

ing to various influences. Can we say the same of black and white as of red and the other colors? I will first describe some experiments that answer this question, after which a number of familiar illustrations will be given.

The apparatus, which is not difficult of construction, is shown in figure 6. A wide tube *a b* is painted outside and inside with Paris black and is inserted through the door of the dark room so that the wider end is outside while *b* is in perfect darkness. Four diaphragms, *c*, *d*, *e* and *f*, are placed in corresponding slits in the tube in such a way that an eye looking through the tube sees an opening about two cm. square. They are placed at a distance from one another so that even the small quantity of light reflected from the walls of the tube is kept back from the neighborhood of the square opening. When in the dark room various colored or colorless surfaces are brought before the end of the tube, the observer at *a* is able to recognize only the intensity and the quality of the light reflected from the surface as long as the surface is so held that the edge of it cannot be seen within the square. By changing the illumination (lamp-light, gas-light, daylight), by changing the position of the reflecting surface to the direction of incidence of the illuminating rays and finally by the interposition of colored and gray glasses, the quality and intensity of the reflected light can be changed at will without the observer knowing anything more than the actual change on the surface observed. The observer was called upon to name the various lights and colors seen through the opening. A series of so-called blacks, e. g. black cardboards, dull black paper, Paris black, etc., was first presented before the tube so that nothing more than the surface was seen. Dr. Leitzmann, who was kind enough to serve as observer in these experiments, was called upon to name the colors. He did not designate a single one as black, but every one as gray, dark gray, a trifle light, somewhat light, rather light, etc. The term black was used only in the case where the opening was completely closed by the diaphragms. Even the best Paris black and the still blacker shadow that can be cast on it were perceived as light; indeed, by very strong illumination they produced the impression of light gray, and on one occasion, by a sudden change from daylight to gas-light, that of yellow. White papers and cardboards were not recognized as white, but, according to the illumination, as light, light gray, very light, light yellow, etc. With great decrease of the illumination they were gray, dark gray, very dark. Only once was an impression recognized as white, and this was from a piece of dirty gray but strongly illuminated wrapping paper. On the other hand colored papers were always correctly recognized, provided the saturation was sufficient for the contrast with the black field of vision not to render the color entirely unnoticeable.

This fact, that our blacks and whites are in reality only grays, is seen in many every-day experiences. That we name an object black or white rather than gray does not depend alone on the quality and the intensity of the sensation of light, but essentially on the fulfillment of other conditions that have nothing to do with the sense of sight. Here we find not only the various properties of the object itself, but also two entirely subjective factors. The first of these is the peculiarity of the sense of sight mentioned in a previous treatise,[1] by means of which on the foundation of the intensities in the field of vision we form for ourselves a maximum of brightness that is not to be exceeded in our reproduced ideas until brightnesses of greater strength enter the field of vision. On a dull autumn day let the eye be fixed on a sheet of white paper and try to imagine fresh fallen snow in a degree of brightness exceeding that of the paper; it will be difficult or impossible, because the memory of the snow is too weak to compete with perception. The case is different when in winter we come directly from the snow-covered street into the room and make the experiment, because what is said of the brightnesses in the field of vision of course holds good also for the very lively memories of strong impressions which have just disappeared. Exactly the same can be shown of the saturation of the colors. Upon looking at a sheet of the best quality of red Heidelberg paper we cannot succeed in imagining a still finer, more saturated red, although the pigment does not produce the highest degree of possible purity and saturation. Of this we can easily convince ourselves, if we compare it with spectral red or even only with red glass by transmitted light or with the best silk velvet. This relativity of the maxima of brightness and saturation—which is to be regarded as a necessary result of the general law of relation—is only one reason why we are inclined to call brightest and most colorless objects in the field of vision by the name "white" and the darkest and most colorless objects by the name "black."

The second of the above mentioned factors is the ever-ruling inclination to correct the present sense-perceptions by connection with previous contents of consciousness, i. e. out of the present impressions and the memories of previous impressions called forth by the given object or those like it we form a complex of sensations or percepts, which necessitates us, on account of the constancy needful for a later recognition, to abstract from some of the components or at least to give them a less degree of attention. We recognize the domestic cat in every illumination, although the visual presentation can differ to an extreme degree in regard to intensity. We pay no essential attention to the illumination, although the presentation was only possible through it. In other words, we represent to ourselves the things not as we see them at the moment, but as we know that they are. When we ask a four to six year old child to draw a face on the slate, it very often happens that a profile with two eyes is produced. The explanation seems to show the same tendency as that just mentioned. The child has often enough seen human beings in profile, but the eye is the most important part of the face and he cannot represent a human being to himself without the two eyes, and he thus corrects his presentation of faces in profile by the adding the other eye. This is exactly the same as the case of every student of drawing. Errors in perspective are constantly made because the person draws things as they are and not as he sees them. An amateur, for example, in painting a winter landscape will hesitate to picture the deep shadows on the snow and he will draw the snow perfectly white, as he thinks it to be.

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[1] **Kirschmann**, *Ueber die Helligkeitsempfindung im indirecten Sehen*, Philos. Studien, 1889 V. 481.

Black, according to Wundt, is the sensation from the complete inactivity of the retina, but it never occurs in one with normal eyes, since special retinal light is never absent. In external objects the nearest possible approach to black is an extremely dark blue. The best Chinese ink, which appears as the deepest black when drawn as a line on white paper, is readily seen to be a gray when it is used to cover a large surface. Even Paris black, which is the blackest of all pigment, is not a full black, for on a surface covered with this black we can draw still blacker lines with the same pigment. Compared with the inner surface of a black cylinder, the Paris black appears distinctly gray. It is just the same with writing-ink or printer's ink; it appears black to us only so long as it is in small lines or letters or numbers on a bright background, where the simultaneous and the border contrasts are so weakened that the difference between the characters in pigment and the retinal light is not perceived. In short, every case where we believe ourselves to see complete absence of light is due to contrast in brightness.

We designate those surfaces as white, concerning which we have learned by experience that with every illumination they possess the maximum capability of reflection and colorlessness. The swan is white, even when it is in the deepest shadow. Even the shadow on the feathers of the swan no one would call gray, although they are far darker than the lights. A piece of white paper can be held at such an angle to the incident light that it is really not so light as a piece of gray paper beside it, which has a better illumination, and no one would call the former gray; yet we believe that we see the former as white and the latter as gray. This, however, happens only as long as we know what the shades of the two really are. If we do not know this, then we judge the two as we actually see them, just as in the experiment described above.

From these considerations it seems to me clear that in order to attribute to an object the property "black" or "white," we need to know more than the quality and intensity of the illumination of the objects and that these designations are not designations of sensations, but of judgments. Black and white are thus concepts and not sensations. There are white objects, but no sensation white, what we call white being simply a gray, with a surface that possesses the maximum degree of capability of reflection under the various conditions of illumination.

3. *Effect of Colored Illumination.* It is often said that in the evening by lamp-light the yellow objects appear almost white and are scarcely to be distinguished from white. This, however, is an illusion of the judgment; just the reverse is true. In reality the white objects appear yellow and the yellow objects still more yellow in the light of the lamp which is rich in red and yellow rays, but lacking in a corresponding quantity of blue rays. This is clearly seen when a piece of white cardboard is so arranged that half of it is illuminated by daylight and the other half by lamp-light, with the appropriate protection from extraneous light. The half lighted by the lamp appears decidedly yellow or pale orange, and yet in the evening under the same illumination, when we have nothing really white with which to compare it, we call it white.

Just the same thing happens in looking through weakly colored glasses, or sheets of gelatine. We believe ourselves to see the objects in their proper colors, as though we could see one color through another. This supposition is incorrect, as is seen by looking at colored surfaces through the apparatus mentioned above in such a manner that neither the edges of the objects nor the character of their surfaces is seen. The mixed color produced by the color of the object and that of the glasses will then be seen and the illusion will disappear. The knowledge of their true colors which we have from general experience has led us to believe that we see the objects in their usual colors in spite of the influence of the glasses.

This explanation, however, is sufficient only in the case of slightly colored glasses. With glasses of more saturated colors there are also two additional factors that show themselves, the fatigue of the retina and the purely psychological influence which assists the recognition of the colors of the objects and an abstraction from the color of the glasses.

With a pair of spectacles, specially made by Krille in Leipzig, in which various colored glasses could be set and which were provided with side screens to keep out extraneous light, I was able to perform in a convenient way all the experiments described by Fechner in his essay on contrast.[1] My results do not differ essentially from his. When the spectacles are provided with blue, violet, yellow or green glasses, after a short time the impression of color disappears and the objects appear in their natural colors with the exception of those objects whose colors are extinguished by the glasses. Black and white objects appear black and white in spite of the fact that only colored light reached the retina from them. The color of the glasses is noticed only in looking at very bright objects.

The color disappears still more quickly when two different glasses are used, e. g. blue for the right eye, purple for the left, or yellow for the right, violet for the left, at the same time taking care that the colors are of about the same degrees of brightness in order that the disturbing rivalry of the two fields of vision may be avoided. In such cases the illusion from the judgment goes so far that the objects are supposed to be in their proper colors and it is generally impossible to say before which eye the blue, before which the purple, glass is.

The fatigue of the retina is not the only cause of this phenomenon, for the illusion occurs almost immediately and the posterior images that always follow fatigue are either very weak or entirely lacking. Moreover, the fact that we do not judge objects according to their intensity and the qualities of their lights is also not sufficient to explain the phenomenon. There seems to be another influence of purely psychological character, arising from the relation in which the sensations of color stand to one another and being a direct result of the general law of relativity. Here we have to do with a lack in the relation of the color of the glass to other colors. If we hold a blue glass before the eyes, it is only in the first moments that we have any occasion to compare the illumination of the objects seen through it with the normal illumination.

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[1] FECHNER, *Ueber die Contrastempfindung*, Ber. d. Kgl. sächs. Ges. d. Wiss., math.-phys. Cl., 1860, 71.

The longer the new illumination continues, the fainter the relation to previous illumination becomes, while there is no possibility of a comparison with the usual illumination at the same time, since with use of the completely protected spectacles just described the colored illumination covers the whole field of vision.

In these experiments I made the same curious observation as Fechner, namely, that the red glasses were an exception in so far that the impression of the dominating color disappeared much more slowly and in the case of bright objects never disappeared entirely. Nevertheless I do not think that any higher or more enduring sensitiveness for red is present; on the contrary, I believe that the peculiarity has its cause in the physical character of the glasses. A transparent body can

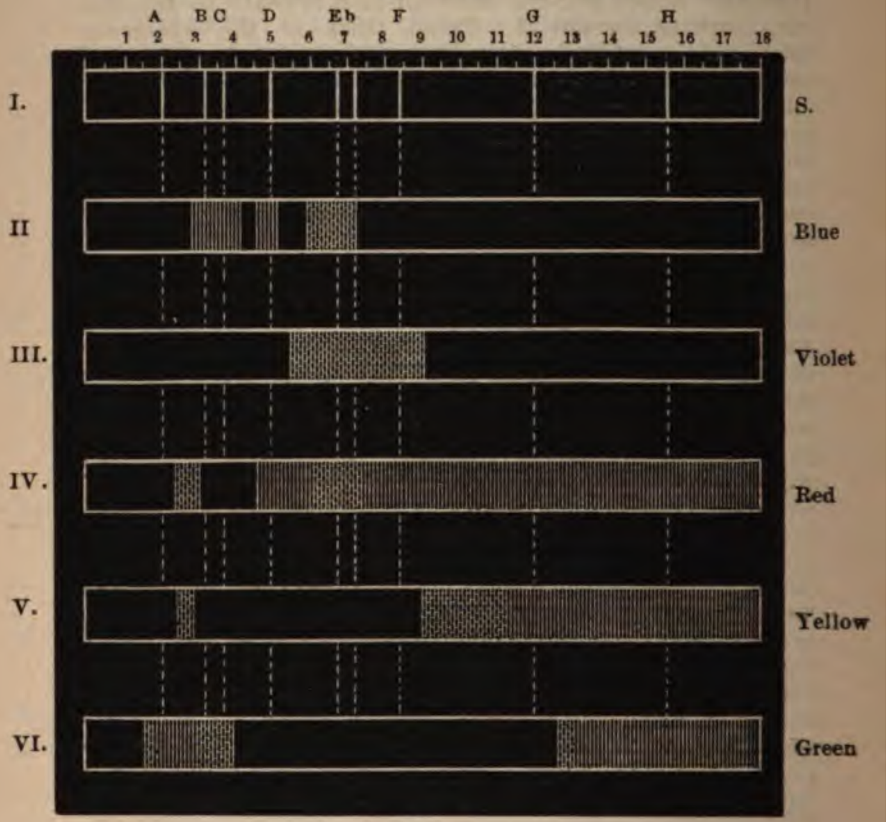

FIG. 7.

possess a color from two entirely different reasons; either when it allows only rays of a certain wave-length to pass through, or when it allows all to pass through except those of the complementary color. A glass can be yellow because it allows only yellow's rays to pass or because all rays pass through it with the exception of the blue rays which it absorbs. The glasses which I used in the experiments just described were examined with the spectroscope. The results are shown in figure 7, where I. indicates the solar spectrum with some of the principal lines, and the others the spectra of various glasses, the color as it appears to the eye being written beside it. Those parts of the spectrum which were completely blotted out are shaded with parallel lines, while those that appeared faintly are represented by crossed lines. It is very clearly seen that the glasses that gave the spectra II., III., V. and VI. (blue, violet, yellow and green) owe their colors as seen by the eye to the fact that the complementary color is eliminated, while the remaining colors are allowed to pass, although some of them are weak. The red glass, however, (superficial oxide of copper coloring) has its spectrum cut off sharp before the D line and with the exception of a light gray glimmer in the place of green permits only light from $\lambda = 0.0007258$ to $\lambda = 0.0006026$ to pass. In consequence of this its effect on the eye is quite different from that of the other glasses. The others do not weaken the light from the objects to any great degree, with the exception of those in the complementary color, and consequently their relative relations of brightness remain almost undisturbed, so that we easily recognize the colors and shades. On the other hand the red glass causes the greatest disturbance in the relations of intensity of the colors of the objects. All red objects appear bright, while all those otherwise colored are seen to be much darker, often almost black. This difference is made still greater by the contrast in saturation between these two classes. Both these influences render it difficult or impossible to abstract from the red coloring and to judge objects in their usual colors, as is done with the other glasses.[1]

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[1] This explanation could be tested by use of spectacles fitted with blue, green, yellow, etc., combinations of gelatine sheets prepared in the manner described by Kirschmann, *Ueber die Herstellung monochromatischen Lichtes*, Philos. Studien, 1891 VI. 543, and so arranged that these colors are pure spectral colors like the red. The result ought to show that such colors have the same effect as the red. On the other hand red plates produced by the absorption of the complementary color ought to act as the blue ones described in the text.

E. W. S.

4. *The Idea of Polish.* Contrast has also a large influence in the idea of luster or polish. I say "the idea of polish," for the polish is not directly given as sensation, but is a product of the combination of sensations.[1] According to Wundt, polish is to be regarded as an incomplete mirroring, whereby under mirroring we understand the regular reflection of light from a plane or a curved surface. Polish rests upon the reflection of the light, which, although not complete like the reflection from a mirror, (where instead of the mirror we see only the object mirrored) nevertheless is not so irregular as the usual dull surfaces. With dull surfaces the light which comes from any direction is strongly reflected in all directions, while in a mirror only one direction of the reflected rays corresponds to each direction of incidence. Between these two extremes there lies the whole series of possibilities where light is reflected in all directions, but one direction is particularly favored; these belong to the category of luster or polish. The favored direction in which the light is reflected depends on the physical conditions, on the number, the position and the size of the sources of illumination, on the form of the shining surface, but is completely independent of the eye. Therefore with even the smallest movements of the eye we see displacements of the relations of intensity. Just this fact is for us the criterion of polish. Likewise the binocular observation of the object enables us to judge of luster, since the two retinal images have their maximum intensities in different places.[2] To this we must add, in the case of bodies of a luster approaching mirroring, the influence of the doubtful fixation of the visual axes and of accommodation.

Nevertheless even in cases where these conditions are not fulfilled, where every influence of the mutual support or rivalry of the fields of vision or the movements of the eyes is removed by monocular observation with completely unmoved eye, and where the object is so far distant that the insecurity of the accommodation and convergence of the visual axes can no longer come in question, the polish is in many cases still present. The explanation of these cases has therefore to be sought in something else than the influences mentioned.

In such cases we recognize objects as polished that send out a not completely diffused light when under a single illumination; the brightness of various parts of the surface stand in such a relation as, according to our experience, cannot come from an irregularly reflecting surface. It is here a question of considerable differences of brightness between adjacent places of the surface; this is a matter which can be essentially influenced by contrast, especially when we are unacquainted with the source of illumination.

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[1] WUNDT, *Beiträge zur Theorie der Sinneswahrnemung*, 315, Leipzig and Heidelberg, 1862.

[2] On the relations of between binocular contrast and polish, see WUNDT, Phys. Psy. II. 179, Leipzig, 1887.

Bodies that completely disperse the light show no differences of intensity on approximately even surfaces with a single illumination and only moderate variations on curved surfaces; the passage from light to shadow follows in a manner completely dependent on the form of the surface. As soon, however, as we perceive greater differences of brightness on a surface of a single object than would be possible with a diffuse reflecting surface, we say that the object has polish. These differences of intensity according to the prevailing maxima of brightness are produceable not only by direct differences of intensity but also under favorable circumstances by contrast; thus in monocular perception of polish we are exposed to many illusions. For example, a gray ball of dull surface can be so painted with black and white pigments that by monocular observation and unmoving eye, it cannot be distinguished from a polished, unpainted, gray ball. Movement of the eyes or binocular observation, of course, destroys the illusion. This in the weapon which in the hands of a painter enables such startling representations of polished objects by means of pigments.[1]

5. *The Emotional Tone in Contrast.* The part played by the emotions in relations of contrast is scarce less than the usual qualitative and quantitative properties of the sensations. Lipps, indeed, regards this factor as the only psychological contrast, others being physiological.[2] Nevertheless it is easily seen that the contrast between the emotions accompanying the sensations, besides the actual emotional contrasts of the sensations, being to a certain degree dependent on it without the intensity and quality contrast being bound to it with like necessity. The sensations of light can be so chosen that they are totally indifferent in regard to their emotional tone, although they are otherwise totally unlike; on the other hand an emotional contrast separated from every contrast of brightness and quality is inconceivable.

The emotional contrast, however, although at present not accessible to quantitative measurement, determines the æsthetic effect of the colors and their combinations as a result of the simultaneous contrast of the sensations. The difficulties of establishing fixed rules for this action of the emotional contrast rests chiefly on the fact that all the kinds of contrast, those of brightness, of saturation, of color and of the emotional tone, each of which is itself variable, can take part in the result, all of which influences must be taken into account. It is a fatal error to judge the æsthetic effect of a combination of colors from the standpoint of the color-contrast alone. It is an absurdity to make such a statement as, "green and blue do not go well together;" it should be, "a certain green and a certain blue when combined in certain degrees of brightness and saturation make a disagreeeble impression." The color-contrast between green and blue is small, but it can be replaced by one or both the other two kinds of contrast; an example in point is the beautiful contrast between the weakly saturated and intense blue of the sky with the highly saturated dark green of the forests or with the yellowish green of the fields. The reverse of the usual statement can be made, namely, that the contrast between any two colors can produce an agreeable effect, provided the contrasts of brightness and of saturation are properly chosen. The maximum emotional effect is reached when all the three—color, brightness and saturation—are properly contrasted.

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[1] See also KIRSCHMANN, *Die psychologisch-ästhetische Bedeutung des Licht- und Farbencontrastes*, Philos. Studien, 1891 VII 362.

[2] Grundthatsachen des Seelenlebens, 273.

6. *Preventive Effects.* Although attention is generally turned to the strengthening effects of contrast, it is nevertheless true that the case often occurs where contrast hinders us from detecting a difference between two qualities or intensities of light. Such a case of the hindering effects of contrast is found in the fact observed by Arago and Hankel,[1] that two unlike but not very different intensities are more easily judged on a moderately bright background than on a darker one. The cause of this is not, as Fechner thought, the "reversed" contrast (the so-called light-induction), but is simply the brightness-contrast. The two small objects (in the Hankel-Fechner example two small slits) are so much raised in their brightness that they come very near the maximum of brightness, whereby their very small difference disappears.

An exactly similar experiment can be made as follows: India ink has a brightness of $\frac{1}{23}$ that of a certain white paper; lampblack has a brightness of $\frac{1}{66}$ compared with the same paper. That is, the former reflects nearly three times as much light as the latter, as can be seen at the first view when the two colors are spread beside each other in large surfaces. Nevertheless if we draw two thin lines on white paper, one with India ink, the other with lampblack, we find that they show no difference, the contrast in brightness with the white paper having reduced them both nearly to the minimum of brightness.

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[1] FECHNER, *Ueber die Contrastempfindung*, Ber. d. Kgl. sächs. Ges. d. Wiss., math. phys. Cl. 1860, 90.

Another effect of contrast is the so-called light-induction. This is, however, not to be regarded, like the usual cases of contrast, as a constantly occurring factor of every perception of sight, but as an exceptional case which occurs only after long fixation. This phenomenon has up to the present been investigated only as the accompaniment of successive and contour contrast. In these cases it is probably to be explained physiologically, as brought about perhaps by the phenomena of fatigue and subjective changes occasioned by the exertion of the organs of fixation (movement) and accommodation or by the interocular changes of pressure, or perhaps by the extension of the stimulation to neighboring parts of the retina. Whether such phenomena of light-induction occur as results of simultaneous contrast is a question that has not been answered.

There is, however, a phenomenon that at first sight might seem to be a light-induction, but is really to be explained otherwise. When we steadily fixate a bright object on a dark background it gradually loses its brightness, while the background becomes lighter. This is a necessary result of simultaneous contrast and we do not need to introduce a new property of the organ of vision. The dark background in the neighborhood of the bright object is really so dark only by contrast with the object. Even a moderately bright object illuminates not only the corresponding portion of the retina on which its image is projected, but also all the rest of the retina on account of the reflection from the retinal image, on account of the very imperfect refraction of the lens-system, and finally on account of the very imperfect opacity of the eyelids and the portions of the sclerotica and chorioidea around the pupil. Even with closed eyes remarkable quantities of light penetrate through the lids to the retina; with open eyes the amount spread over the retina must be very much greater. This brightness of the whole retina generally escapes our notice on account of the contrast with the direct illuminated portions. By long continued fixation of a bright object, however, there occurs a progressive fatigue which, on account of the impossibility of a comparison with other objects not under the same conditions, we are aware of only through the change in the relation of the intensities of the object and the background. But the more the actual intensity of the bright retinal picture decreases the less becomes the effect of the contrast on the dark background. This consequently appears to become lighter, since its illumination, derived in the ways mentioned above, takes a more prominent part when not repressed by contrast. Exactly analogous is the case of a colored object on a dark background.

That contrast is able under certain circumstances to repress the otherwise perfectly apparent difference, is seen in the following experiment. Let a small circle be painted with a very thin solution of carmine or ultramarine; it will be of a dull pink or a bright blue color and will be quite distinct from the white background. Now, with the same color but in such saturation that it covers the white thoroughly, paint around it a ring of a thickness equal to the diameter of the circle; the circle will appear to be as perfectly white as the background. In this experiment we have to do with a simultaneous contrast in saturation and brightness; the former brings it about that we do not perceive the color of the inner circle and the latter prevents the inner circle from appearing darker than the external background. That the brightness-contrast exercises a stronger effect on the inner circle than on the external background is partly the result of the fact that the background is nearer the maximum of brightness and can be made brighter only to a small degree; it also depends on the relation of the strength of contrast and the extent of the contrasting objects.

7. *Recognition of Objects.* It is a matter of great importance to determine if the general psychological law, that we judge impressions not according to their absolute values, but according to their relations to one another, works favorably for the accomplishment of the functions of the sense of sight or not.

The duty of our sense of sight lies evidently not in perceiving absolute intensities and qualities of light, but in recognizing and naming familiar objects under the most varied circumstances, and in giving unfamiliar objects their proper positions among the familiar ones. For these purposes the brightness and the color of bodies are the most important, since we can recognize their outlines and size only by help of quantitative and qualitative variations of light.

This is readily shown by an example from common life. Suppose, when we look into a room on different occasions, we see it under continually different degrees of illumination. Every object reflects a different amount of light on each occasion. Yet this in no way affects our recognition of them. We recognize them under the most varied conditions. For recognizing them we make use of their outlines and their *relative* intensities of illumination.

The absolute intensities and colors of objects, the surfaces of which, according to their physical characters, always reflect only a small percentage of the light falling upon them, are exceedingly untrustworthy factors of the visual percepts, on account of the extremely frequent and extensive change of illumination, and are therefore the worst means imaginable for the recognition of objects. In case, however, sensations of brightness and color are to assist in the recognition and determination of objects by means of their degrees of intensity, then the eye can measure not according to absolute but only according to relative standards, since the relation between the brightness and color of an object and that of its surroundings remains the same within considerably extended limits on account of the almost complete constancy of the co-efficients of reflection, which are valid for the kinds of light possible in common life.

If we were to estimate according to absolute intensities, the sensations of brightness would be of value for the recognition of objects only in so far as they provided an easy perception of the outlines of the objects and their parts. Every variation of illumination, however, would change essentially not only the whole picture of the objects in the field of vision but would also disturb and distort the relations of brightness of these objects to one another up to the last degree of indiscernibleness.

Since we are, on the contrary, under the greatest possible abstraction from or removal of the recognition of absolute differences, and are provided with the ability to recognize and to estimate not intensities themselves but their relations to one another, therefore we are placed in a position to recognize objects through the relations of brightness of their surfaces in as far as the co-efficients of reflection remain constant by variable illumination.

It is to the law of relation that we owe the possibility of carrying out this chief function of the sense of sight, namely, the recognition and the determination of the objects of the external world. Indeed, we can say "contrast" in place of "law of relation;" for contrast is merely the expression of our conviction that the same intensities of the stimuli possess different worths as sensations according as they enter into various relations to other impressions; or, in other words, that the difference of two sensations cannot in all cases be referred to the difference of the stimuli which cause them, and that therefore from the likeness or unlikeness of sensations and ideas we cannot judge the likeness or unlikeness of the objects unless we take the whole contents of consciousness into consideration.

REPORT ON AN EXPERIMENTAL TEST OF MUSICAL EXPRESSIVENESS.

BY BENJAMIN IVES GILMAN.

During the latter half of last March I sent to a number of residents of Cambridge and Boston who are interested in music a circular of invitation, from which the following sentences are an extract:—

> AN EXPERIMENTAL CONCERT.— May I ask your aid in an attempt to make an experimental contribution to the question so much debated regarding the power of music to awaken definite ideas and emotions in the listener. It has occurred to me that some results of value might be reached if a careful selection of musical fragments, to which a definite expressiveness has been attributed, were to be performed in the hearing of a number of persons interested in music, who should previously agree to set down independently of each other the impressions they receive therefrom. Should a sufficient number of those to whom this notice is sent signify their willingness to join in such an undertaking, I propose to give what may be called an experimental concert, on some evening to be hereinafter selected. A lack of theoretical knowledge of music need in no way be regarded as a disqualification for the task proposed. On the contrary those who *feel* rather than *understand* music are to be considered to be the best audience for such a purpose. An interest in the purely musical aspect of a composition might hinder rather than help that imaginative grasp of it which it is here desired to test.
>
> I propose to obtain the listners' judgments in the form of answers to a question prepared beforehand on each of a number of musical selections, which appear in a definite way expressive either to myself or others.
>
> The questions will be numbered and each of the listeners will be provided with a notebook, in which he will be asked to jot down his reply (to be numbered to correspond) without consultation with others, during a pause after each selection. Each listener will be asked to write down the name of any one of the fragments which proves familiar to him.
>
> The undertaking is so entirely novel that it is difficult to predict its outcome, but I am sure there are many among us whose imaginative interest in the art of tone and power of giving this interest expression are capable, if the right opportunity be offered, of throwing a valuable light on the vexed question of musical expressiveness.

About thirty persons were kind enough tó accede to this request, and on the evening of the 29th of April the experiment took place in a parlor in Cambridge.

The instruments used were a grand piano (from the Mason & Hamlin Co., Boston.) and the violin. The interpretation of the programme was intrusted to three well-known musicians of Boston, Mr. Charles L. Capen (piano), Miss E. M. Yerrington (piano) and Mr. A. van Raalte (violin). I am glad to express my appreciation of the interest in the undertaking displayed by these artists, and my thanks for their skillful coöperation.

The whole company, performers and audience, began the evening in a very sceptical frame of mind regarding not only the value of any data which might be obtained, but even the possibility of carrying out such a test. The result belied our forebodings. The method of inquiry proved a practicable one, and there was, I think, a general feeling of surprise among the listeners at the amount of booty rewarding their determined efforts to capture the suggestions of the music played. I may be permitted to express my personal belief that only very rarely indeed would it be found possible to enlist in such a cause as much ingenuity, candor and good-will as was shown by the subjects of this experiment. It was expected that several musicians by profession would be among their number, but as it turned out the audience consisted entirely of amateurs. A large minority, if not a majority, of these were without special skill on any instrument; a few were distinctly non-musical in the sense of having no marked endowment of musical ear or memory; but there were none present, I think, who were not capable, at least at times, of enjoying and feeling music deeply.

The work of the evening consisted in obtaining answers to fourteen questions based upon thirteen selections of music, one being the subject of two questions. Nearly all of the pieces were played more than once, some several times, and although they succeeded each other almost without intermission, except for putting the questions and making necessary explanations, the experiment lasted without any relaxation in the interest of the participants from eight o'clock until about midnight. Twenty-eight notebooks were the result, sixteen contributed by gentlemen and twelve by ladies, the former being indicated in the transcription below by Roman, the latter by italic capital letters. Each listener replied on an average to about three-quarters of the questions. The contents of the notebooks here follow (one written in German is translated) appended to the several questions, with each of which is given the particulars of the music to which it refers and the expression of opinion on which it was founded. Two of the selections were fragments of elaborate concerted compositions, and as it proved that the suggestion of these, which alone could be given by the piano and violin, did not afford sufficient basis for judgment, they are not here reported upon.

As one of them was the subject of two questions the following transcription consists of the replies to eleven questions on eleven different selections. These are all either piano compositions or melodies written for the voice with or without accompaniment. The notes are given as they were handed in at the close of the evening, with changes (to carry out the intention of the writer either evident or since ascertained) in perhaps not more than a score of words in all. The selections are understood to have been unrecognized unless the contrary is expressly stated.

QUESTION I.

Give any image that is strikingly suggested to your mind by the course of the following piece.

Beethoven. Pianoforte Prelude in F Minor. It bears no opus number, but in the Breitkopf and Härtel edition of Beethoven's Works, is No. 195 in Series 18, "Kleinere Stücke für das Pianoforte."

To the writer its character is that of an unending contest with an opposition that bars every advance. It is an attempt to hew a way through adamant. We could fancy ourselves listening to the tireless dialectic of a mediæval schoolman on some transcendental thesis, or even admitted to the mind of a melancholiac eternally resenting miseries eternally visited upon him afresh. Dry and gloomy energy doing doughty deeds to no purpose is to me the burden of the piece. Piano solo.

ANSWERS TO I.

A. The swaying of the treetops in a moderate wind; weird songs are sung beneath the trees.

B. A country church appeared to me; the music formed the chimes; the surrounding scenes were grave or gay as the music became slow and soft or fast and loud. As it died away a funeral train seemed passing.

C. No image. Technique (not of performance but of composition) entirely covers up the æsthetic effect. I cannot help being lost in the sequence of the strain, especially on an instrument of percussion like the piano.

D. At first, organist seated at organ in church, then a change at end to twilight; a large hall; a man who has felt sorrow, yet feels the grandeur of life above all, improvises; a love sadness.

E. Plunge of a torrent in the woods; then children's feet dancing as the key changes; sunburst. Thenceforward the piece gets more dramatic, forming a sort of tumultuous dialogue or inward dilemma of affirmation and negation. It rolls on some practical moral decision, and with moments of peace or weary diversion it ends in a sort of forgetting calm without particular triumph.

F. A hymn of thankfulness.

H. Persistent struggle with rather mild difficulty, e. g., walking through a wood with thick underbrush.

I. Chime of church bells; bright, sunny morning; gathering to church; in church; entry of minister; hushed; minister rises; ready for service; last stroke of chimes.

J. The rolling up of breaker after breaker on the beach with the sound of more distant rollers in the lulls; or the dying away of a storm.

K. This is a fugue. Fugues always suggest to me the beauty of organism, the universal not being built out of an accretion of particulars but revealing itself in subtle relations among them. The complexity of law. The essentialness of sadness to happiness.

L. A great strife against something; a final conquering of this something and then rest. This strife seems to return at times and is then quieted; finally, near the end a burst of it and then the quiet closing bars.

M. Suggests a life toiling on through disappointment and struggle, until at last peace comes, a peace of which there had been moments of anticipation. Not a brilliant or a prominent life.

N. The resolute self-possession of the process that is going on suggests at once something very much alive, very free—a nature force in full possession of its own world: Sie entlässt sich frei, ihrer *selbst ganz sicher*, says Hegel of the *Idee*, when it passes over into *Natur*. I have a sense that a water-process would be the scene most naturally suggested. Scene, however, not complete, but waves on water most probable.

O. A rather distinct idea of a workman making something by strokes, as a smith. There is also a feeling that he is in a lazy mood, as if the afternoon sun were streaming in. The work is pleasant.

P. Church; opening voluntary. Religious cheerfulness. A religious dance; measured movement of hands. Or, somewhat, a brook tumbling along over a stony bed. The suggestion of a yearning.

A. (Bach.) A ship approaching end of voyage; all tension; haven.

C. It (the piece) seemed to me to embody the progress of a mountain stream on its course from the hills to the plain, flowing among rocks over many obstacles, under the forest trees, with the quiet and deep repose of the wild wood pervading all. This was the only image that occurred to me. The intensity of the stillness of the wood was most prominent.

D. Persistent effort, resulting in serene progress.

E. A perpetually struggling bird, flying up and beaten back by the wind.

F. Beating of the waves upon the rocks in the receding tide.

G. Storm wind; agitated sea; dashing on rocks or through pines; increasing, then gradually subsiding. A rock-bound coast with weather-beaten woods, mostly pines.

Spiritual vision: Strong emotion; unrest; doubt; gradual peace, though not joy.

H. The last part makes upon me the impression of a scene of farewell, and I seem to see the departing friend disappear beyond a hill.

I. Dark clouds; storm. An old German church with a suggestion at the close of a funeral service over some great and heroic character. A feeling throughout as of a strong resounding sea against a frowning coast.

J. A controversy or argument between a man and a woman, ending in a great peace.

K. The incoming tide dashing on the rocks, with intervals of quiet ebb.

L. Church music; offertoire; also, organ playing while waiting for a wedding party; cheerful, and not too joyful; serene; also, the suggestion of hearing the organ playing inside, while outside, in the summer. (Bach.)

QUESTION II.

The two melodies to be played are said to be in a certain respect opposite in character. What particular form of contrast, if any, do they in your opinion embody?

First eight bars of "O, mio Fernando," aria from the third act of Donizetti's La Favorita, and first five bars of "Durch die Wälder," aria from the first act of Weber's Der Freischütz. In "The Power of Sound" (London, 1880), p. 108, Edmund Gurney applies to the melody from La Favorita the words "flaccid feebleness," and to that from Der Freischütz the phrase "serenely and lastingly fair and strong." Violin solo.

ANSWERS TO II.

A. (1.) An expression of sadness and love.
(2.) Light-hearted, triumphant affection. Or,
(1.) Liebesweh.
(2.) Freude.

B. Despair; a plea for mercy versus relentless triumph.

C. Distinctly opposite; something like yearning and satisfaction (in the same range), or doubt and assurance (in the same range), or anxiety and relief.

D. (1.) Resignation.
(2.) Gaiety with a touch of something else. Or,
(1.) A gray haired woman sitting at a window.
(2.) Some touch of laughter.

E. (1.) Plaintive, looking to past.
(2.) Joyous, looking to future.

Number 1 seemed to me at first to express a noble *resignation*, but later I was uncertain whether *longing* was not more strongly there.

F. (1.) A farewell, or a regret.
(2.) A greeting, and an expression of delight.

G. (1.) Plaintive.
(2.) Joyous.

H. The first piece is poetic, melancholy, moonlight music. The second joyous, though with a certain seriousness, and is full of sunlight.

I. The first melody was sad, depressed, longing, regretful, *relaxed* body. The second was joyous, buoyant, expectant, up and doing.

J. Sorrow without hope. Sorrow with hope.

K. (1.) The daughter of a Teutonic chief pleading with a Roman general for her father's life.

(2.) Triumphant leadership; command. Contrast: The earnest seeking of an essentially feeble nature, and the joyful success in attainment of a nature that is strong.

L. There is a sense of unsatisfied longing about the first. The second seems vigorous and full of hope. The first is dependent, the second independent, in general effect.

M. First selection reminds me of moonlight on the ocean; calm; a small boat rowed slowly. Second suggests a sparkling breeze; in a sailing boat. (N. B. I am fond of the water.)

N. The well known quotations from two poems of Browning *at once* suggest themselves as characterizing the contrasted moods.

(1.) "That was I you heard last night,

* * * * *

Serving most with none to see."

(2.) "So, I shall see her in three days
And just one night, but nights are short,"

* * * * * *

[The two poems are those entitled, "A Serenade at the Villa," and "In Three Days," in the Dramatic Lyrics.]

O. Discouragement; confidence. Doubt; gay, nonchalant purpose. The contrast is not very definite in my mind; strong, but vague.

P. First melody, plaintive; gently sad, as of a lover leaving his mistress, or a poet who is conscious of missing his ideal, or a girl leaving home to go to school. Second melody, vigorous cheeriness; manly satisfaction; a certain joy and springiness.

A. (1.) Pathos; sadness with hope.
(2.) Allegro; cheerfulness.
C. (1.) Self-controlled emotion.
(2.) Impetuosity.
D. (1.) Looking backward with sadness and regret.
(2.) Looking back on happiness.

E. The first suggests a wandering about in dark places. The second, a buoyant spring upward into regions of light. The shallowness of both strivings is felt.

F. The first expresses hesitation. The second, resolution or achievement.

G. First, a petition, as of one doubtful of the favorable response, but beseeching a hearing; not devotional, but human, as of a lover and his mistress. Second, a reply—not of a love-lorn maid, but cheerful and slightly upbraiding.

H. (1.) Evening peace.
(2.) The freshness of morning.
(1.) Quietude of soul.
(2.) An awakening to new energy.
I. (1.) Sentimental.
(2.) Lively.

J. The first is seeking, the second expresses attainment.

K. A contrast as if a mood of somewhat sad meditation beneath the sighing pines were cast off and serene joy took its place.

L. (1.) Serenade; hopeful.
(2.) Window opens, all is joy; he feels she cares for him.

QUESTION III.

What is the main impression produced by the following passage taken as a whole?

Beethoven. Piano-forte Sonata in D, opus 28 (often called the Pastoral Sonata, but, it is said, without warrant from the composer). Fragment of the allegro, beginning with the 77th bar, and ending with the 125th. According to Edmund Gurney (Power of Sound, p. 169), this passage "affects the inner sense with a compulsion, a concentrated passion of movement, so overpowering that I scarcely know its parallel in music; the four bars break in the middle, making the swing of the motive, as it recurs, seem more than ever resistless." The allegro was played from the beginning up to the 136th bar, the attention of the audience being especially called to the passage remarked upon by Gurney. Piano solo.

ANSWERS TO III.

A. Joyful contentment.

B. The piece brought to my mind a girl half talking, half singing to herself, ending with a careless laugh.

C. Very vague; but something like the joyous feeling of out doors, with its invigorating and cheering influences.

D. It suggests the opera; the orchestra works to a mild climax; not the grand climax of the whole. A woman sings one of her *first* songs; a touch of feeling ending with the customary runs.

E. Noble joy on a terrace, eighteenth century, people in pearl color and powder dancing it; then the piece loses that date; the joy appears based on an assured good impatiently looked for in the rapid running passages, and the reasons of the certainty of its coming rather triumphantly laid down in the staccato thumps.

The above describes the whole piece, not only the termination.

F. The coming of spring.

G. Suggests a melody in one of Sullivan's operas, perhaps Iolanthe: "I heard the witch remark, etc."

H. Impression very slight; mild progress and success; runs, to me, are meaningless.

I. Song chorus; jaunt on the cars; singing to the beat on the rails; no deep emotion.

J. Gave me a feeling of light-heartedness, such as one in perfect health has in the early morning of a beautiful day; the joy of life and nature.

K. Renaissance work. Trivialness of surroundings; flounced dresses, hair powder, coats with long skirts silk lined, elaborate walking sticks, —and human hearts beating, life real in it all.

L. This (or something that suggests it strongly) is familiar to me, and as I have entirely material associations with it, I cannot disconnect them from the music. As far as I can do so, it seems at first a little trivial, becoming then more serious, with occasional outbursts of the trivial side.

M. It puzzles me. The impression is filled with charm, but is very difficult to analyze. Suggests something slightly frivolous. A comic opera?

N. Beethoven "Pastoral Sonata." My impressions are very old and personal as to this sonata. I never found especially "pastoral" associations, as such, in the first movement, although I always imagine myself in the open air, under blue sky. But that is arbitrary. The passage in question has purely religious context otherwise in my feeling; the climax of a moment of cheerfully adoring resignation, voluntary abandonment of finitude, with a certain insistent and repeated delight in laying off, as it were, the clothes of one's soul before taking a very jolly flight into the blue.

O. Nothing clear.

P. Dance of village young men and maidens; pleasant or gay responses; mild abandon.

A. A child learning to walk: Step high; step low; faster; ha-a! run!

B. Serene confidence.

C. Undefined.

D. The joyful consent of many.

E. Known: Beethoven. A wavering between two desires, each of which is worthy; now one is stronger, now the other, and the decision comes nearer and nearer. It is almost reached when the steps leading to decision all are shattered and have to be retraced. The conclusion of the whole matter is a decision inconsistent with the premises.

F. A vague impression of regret.

G. No clear impression. The first half brought a remembrance of a peasants' fête in Brittany; the last half, nothing.

H. The joyful uplifting of an oppressed soul that feels itself released from depths of anguish through faith in a kind, heavenly Father.

I. No impression other than a musical one.

J. A demand; a bitter disappointment, concealed by gaiety and nonchalance sometimes, but ill concealed.

K. Rocking in a boat on a dancing, sparkling sea; surroundings cause a happy state of mind.

L. No impression.

QUESTION IV.

The following music has been said to tell a certain story. What dramatic suggestion do you find in it?

Chopin. Ballad No. 2, in F major, opus 38. In his recent essay, "Die Musik und ihre Meister (translated under the title of "A Conversation on Music," N. Y. 1892), Rubinstein writes (p. 10): "Is it possible not to call instrumental music a language? Of course, if the first movement be rendered merely in a lively tempo, the second merely in a slow tempo, and the third merely in a spirited tempo, the executant feeling no necessity for further expression, then we might call instrumental music non-expressive," and regard vocal music as alone capable of real expression. * * * Another example: The Ballad in F major, No. 2, of Chopin. "Is it possible that the interpreter should not feel the necessity of representing to his hearers: a wild flower caught by a gust of wind, a caressing of the flower by the wind, the resistance of the flower, the stormy struggle of the wind, the entreaty of the flower, which at last lies broken? This may also be paraphrased: The wild flower, a rustic maiden; the wind, a knight; and thus with almost every instrumental composition." Piano solo.

ANSWER TO IV.

A. First hearing. Two lovers are on shipboard and their happiness is interrupted by storm and eventual shipwreck.

Second hearing. Two happy lovers are sailing over smooth seas, the ship is attacked by pirates, who are beaten off. A fierce storm arises, the ship bearing the two lovers is destroyed, and after the storm the sun shines again upon the sea, now somewhat calmed.

B. The piece naturally suggested a *murder*. It opens with a picture of the assassin creeping *slowly* along and you hear the shrieks of his intended victim when he is brought face to face with his slayer. Here the music, now *shrill*, now *deep* and *low*, seems to mingle cries and groans as the deed is committed and the man finally dies. The assassin slowly crawls off again and the lighter tones that are introduced seem to be the ordinary events of life passing on about him, producing much the same effect as the knocking on the gate in Macbeth. At the end the murderer is by himself and the last notes suggest regret entirely unavailing over what has happened. It ends with wild remorse.

C. (General impressions in succession.) Devotional; storm; gaiety; stormy; gentle again.

Absolutely no definite dramatic suggestion. If story known could easily be made to fit, but without that no single suggestion, not even perhaps definite emotions.

D. It suggests no definite story. A quiet life interrupted by some sort of passion, back again to the old theme, with something richer in the harmony of the old, something gained from the interruptions.

Second hearing. I do not fully understand the sadness of the first part, that becomes an undercurrent in the first interruption only to reappear. It might be called a romance. In some way it suggests George Sand and Chopin. Mr. ——— (another listener) tells me his story and I have now the definite picture I lacked.

E. The thumping and haste of the latter one-third or one-half were nothing to me but *intolerably disagreeable* noise, quite meaningless. The first uniform segment of the piece was delicious noise, of which the only dramatic suggestion was the passage through life of a rather rich-mindedly sober and patient sort of man, with one leg shorter than the other. His inner gravity and modesty seemed connected with his lameness. At one point he tumbles off the bank into the water and then, *bang!* is caught in the whirlpool rapids for a long time until he gets out rather wet; after which nonsense and noise, but for the short gleam of sanity at the very end, which is sweet but irrelevant.

On second hearing arbitrary character of noisy half more infernal than before.

F. Pastoral peace; sudden alarm, (why the lull here?) then conflict, bells, fire, night; and rest.

G. The only part which I recognize is a thunderstorm in the middle of the piece.

H. If I had not been told I do not think I should have felt any story; as it is the piece suggested successive batches of dolls out for a morning walk; they meet soon naughty boys, who rip them open and make the sawdust fly; wild war-dance of boys; one lone doll going home.

I. Lullaby; mother; baby; cradle; rocking; love-gentleness; good night. Changes to thunderstorm; fear; rain ceases; lullaby again. New theme thrust into the picture; leads up to a strong emotion; not quite tragic; hope of future for child. Now grows tragic, the sky of hope for the future clouded; trouble, sorrow; all may be a dream of the child's future by the mother; confused gathering to a climax; breaks; envoi.

If a *dream*, it (envoi) is a sad good night; but rather it is now the narrator saying "So it was, and so he died," "So it ended," "That was the end of him."

Played a second time. Lullaby as before, storm ditto, all as before.

J. Peace and quiet—sudden terror—violence, confusion. At the last a climax, as of a violent death, succeeded by silence, a hush. The main impression one of great *contrasts*, sudden changes in scene. Human strife breaking in upon human peace and happiness, or upon the quiet of nature; the whole rising to the climax; increasing rapidity and intensity of action; the catastrophe; the end.

K. A bare heath, or stretch of sand beach; she remonstrating with him, who is going. Then a battle, she seeking among the dead and wounded. Dido and Æneas.

The general form of which the preceding are the content would be: The sad calm of monotony broken in upon by a catastrophe whose succeeding waves overwhelm the soul.

L. The story which first suggested itself and which I can't get rid of, although it seems in places inconsistent, is of two persons at first amicable and then quarreling, finally becoming reconciled only to quarrel again. This seems (is) too trivial for such a magnificent thing. As to the musical enjoyment of this selection, it is very great. The more I think of it the more artificial my answer seems, though I have no substitute for it.

M. Extremely beautiful, especially at first. Early part suggested monastic life, as it should have been, in the middle ages. Then war sweeps over the country and demolishes the monastery. An effort is made to reconstruct the old life (whether it is a tale regarding an individual or a group I do not know), but the attempt is not finally successful. The life then becomes confused, mingles with the crude movements of cruel times, and ends in physical or moral battle. At the last moment occurs a recollection of earlier peace.

N. (My first sketch on first hearing thrown out on second hearing. The first was due to a false reminiscence.)

The hero of this bit of tragedy is in much the same world as Poe's lover in

"Thou wast that all to me, love
For which my soul did pine,
[A green isle in the sea, love
A fortress and a shrine,
All wreathed with fairy fruits and flowers
And all the flowers were mine.

Ah, dream too bright to last!

* * * *

No more, no more, no more,
(Such language holds the solemn sea
To the sands upon the shore)
Shall bloom the thunder blasted tree,
Or the stricken eagle soar.

And all my days are trances
And all my nightly dreams
Are where thy dark eye glances
And where thy footstep gleams,
In what ethereal dances
By what eternal streams."]

(The poem is called "To One in Paradise.") Only he struggles more fiercely for the lost love, has a much more Titanic determination to win her back—but fails.

O. No *coherent* story suggested. At first it seemed as if a young girl were living a quiet passive life. Then an external misfortune, a war perhaps—not a gradual passion of her own—came upon her. The rest seemed a sort of pursuit and effort to rescue the victim. This involves the sacrifice of the old life, which at the time seemed dull and sad, but now seems beautiful.

On second hearing it seems there are three passions or tendencies, a solemn and awful one beside the exciting one. But I can't define what they are.

P. Summer: zephyr followed by storm.

Or hymn within a convent, and a surging crowd or a battle without.

In the soul: conflict with intervals of peace.

A raging soul listening to a distant church-service.

A. Fête champêtre. Children's minuet—in old fashioned dress.

A fierce storm; thunder, lightning; consternation, dispersion. End of dance, gravely.

After interval—a stately procession or polonaise, by the same company grown. A storm of warring passions. Reminiscence of first scene.

B. Disappointment, spirited endeavor, success.

C. The piece suggests to me the following dramatic situation: Firm, calm, resigned acceptance of fate or future by an earnest nature capable of deep feeling, though in opposition to desire; met by equally decided but stormy resistance and expostulation, on the part of an unprincipled and ardent nature, which is unsuccessful at the end, though unconvinced. Love would be presumably the theme.

D. A funeral march or dirge for a young girl chanted by her girl friends. A sudden contest and strife. I thought of Ophelia's death and Hamlet's struggle with Laertes. After long strife the dirge is repeated as if thought centred again about the dead girl.

E. A life capable, sensitive, forceful, recognizes very early its own possibilities for good and evil. The life is a fair one outwardly and the world judges it right in calling it blameless; but the world cannot see the inner mental and spiritual working.

(I.) The soul itself sees beneath its own outer and even inner calm; and recognizes that there is some element here that will work harm; it sadly feels its own powerlessness, or weakness of will to prevent the harm.

(II.) Then the thought of temptation with its horrible significance clashes on the soul; but the anguish caused by the thought repels it at first, and keeps it at a hovering distance, yet the soul knows that the temptation will return. In this first period the soul knows that, mentally, it is stronger than its foe; but in the next stage, the soul is weakened and does not resist; it welcomes all the horror that it hated before and yields fully to the delight of the sin,—all this mentally.

So at the end, as in the beginning, it still appears calm and pure in the judgment of the world.

F. It is the story of a life beginning with a happy peaceful youth, suddenly broken in upon by excitement and adventure, ending finally in a return to the quiet, uneventful life of the early days.

G. First hearing. Scene, a square in a foreign town; cathedral spire; bells; procession of priests; organ music; enter church; sudden inrush of soldiers and people; gaiety, dancing; hush! reappearance of priests bearing a bier; bells and organ again. They pass on and new scenes, of varying character, occur; but all en masse and all in the same square. No *direct personal* drama of individuals.

Second hearing. A something ominous! War! Storm! a gathering in frightened groups. Ah! the bells again. . . . .

H. I hear two antagonistic elements in conflict—the bells of cloisters inviting to tranquillity, and without, the raging storm and strife of men for earthly gain, where the strong conquers and the weak succumbs.

I. The opening movement naturally suggests a gondola, and that of course Venice with its thousand associations of intense and varied life and adventure; but it was Venice that suggested whatever images arose in my mind and not the music especially. Naturally Browning's "In a Gondola" was thought of; also a storm,—with wreck,—and returning calm, but the sullen calm of subsiding storm.

J. Peace and war; peace finally. The first part a mother soothing her child.

K. No story is suggested; but expostulation and sometimes entreaty opposed to a vehement and impulsive expression of something desired to be done at any cost.

L. Practising church music; old chants and quiet chorals not too grand, broken in upon by thunder-storms; boys' voices, and almost relief.

QUESTION V.

How would you describe the general mood which the following music is fitted to incite, or the atmosphere which seems to pervade it?

Beethoven. Pianoforte Sonata in E, op. 109. Andante Molto cantabile. The theme alone without the variations.

It suggests to me a mood of devotional meditation (Andacht).

Piano solo.

ANSWERS TO V.

A. I am still too much under the influence of number IV. to be affected by this number.

B. Doubt; hesitation.

C. Resignation.

D. Peaceful; but sadness in it.

E. Pensive; not passionate—and grave; not regretful. (Nothing more determinate!)

F. Prayer.

G. Proceeds from a placid mood in the presence of the sublime.

H. Religious.

I. Devotional scene; not *very* religious, but dignified.

J. Seriousness, solemnity, thoughtfulness, religious feeling.

K. Reverent, joyful worship.

Strasburg Cathedral; a procession passing along the nave; a choir-boy swinging a censer turns his face and looks at the spectator.

L. Somewhat religious, though it has a shade of vague unrest in it.

M. Religious; suggests some German church music.

N. No impression worth noting beyond a general atmosphere as of a calm introduction to a dignified ceremony (?)—This interpretation seems doubtful.

O. Not sure:—thought still about the former piece. Is it religious peace and resignation?

P. Tender seriousness.

A. Religious expansion; grateful worship of a full free heart.

B. Seriousness of life.

C. Tender religious melancholy tinged with a sense of pathetic pleasure.

D. Placid retrospect.

E. Known. A mood of comfort and endurance born from sorrow.

F. Retrospection.

G. Devotional; religious.

H. Longing after a higher life.

I. Hoch, heilig und hehr.

J. A generous and complete nature.

K. Self control and the quiet happy feeling that follows success.

L. A restless person waiting for some tardy arrival, trying to forget himself in writing out some serious music.

QUESTION VI.

Do you detect anything not commonly depicted in music that finds its expression in this melody?

Mozart. Nozze di Figaro, Cavatina of Barberina "L'ho perduta" in the 4th act. According to Gustav Engel (Aesthetik der Tonkunst Berlin 1884, p. 134) Mozart has introduced *comic* traits into this air.

Violin and Piano.

ANSWERS TO VI.

A. A fading and wilting flower.

B. The wind crying through a knot hole in the attic, as I have heard it in an old tumble down house.

C. A very sad humor. The impression of oddity and pathos is distinct to my mind.

D. Indefinite.

E. Sort of rustic uncouth unwillingness ending with consent.

H. Seems to ask a question.

I. She is told or discovers that he is not true; someone intercedes and says it may not be so. She with dignity but with firmness tinctured with deep grief resents the conduct and is turned against him. (Is not very definite in its suggestion; not clear.)

J. Anxiety; weakness; as of a crying child; relief, as though the mother came.

K. Not uncommon in music. Disappointment; "Well, never mind." "Men must work and women must weep."

The pathos of monotony half felt in the midst of the commonplace.

M. Plaintively beautiful. Yearning aspiration? Cannot think of any unusual feeling expressed.

N. The only impression would seem to me no more uncommon than any other. It is an impression of someone seeking in hope and sadness mingled for some lost thing—whether a lost child or a lost latch-key, I can't say. The search grows more painfully despairing and ends in giving the whole thing up. Even so I have looked in vain through all my pockets for the last nickel and found it not. The search is one made in grief, but there is no high tragedy.

(Certainly not consciously recognized in any way, nor remembered.) Parenthesis added after learning real nature of piece.

O. Nothing clear.

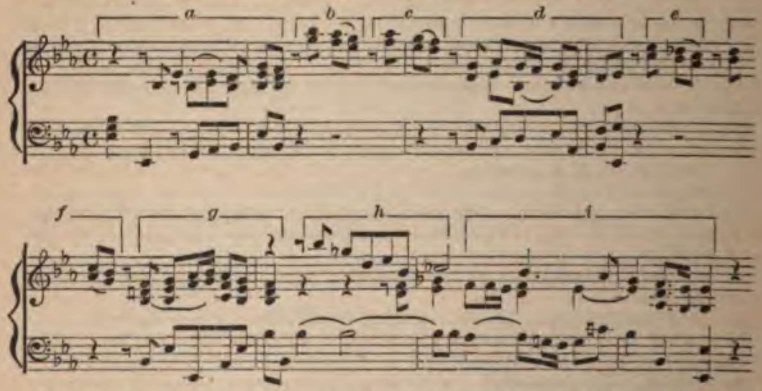
P. Nothing, except, perhaps, a moan; yet the like is common in Mozart, e. g., in his Tito.

A. A sunny Mark Tapley-ism.
B. Anxiety; gentleness.
C. Indecision.
D. Questioning.
E. An every day customariness.
G. Seeking, doubt, questioning, despair.
H. A conversation of lovers in questions. Teasing.
I. Entirely engrossed by the music; no images or impressions.
J. Blindness.
K. The sweet, stern character of a noble woman.
L. A little child talking to her favorite doll, telling her how much she is loved and begging for an equal return of the love; then singing her off to sleep.

QUESTION VII.

Händel's air, "He was despised and rejected of men," from the Messiah, is generally regarded as a musical expression of great sadness, if not dejection. As the successive phrases of its introduction are now played through, indicate which contribute chiefly to its character of melancholy, and why they do so.

The notes of this selection were exhibited on a placard in sight of the listeners, the successive phrases being lettered as follows:



Of this fragment Gurney writes as follows (Power of Sound, p. 273): "In the opening to 'He was Despised,' the pathetic effect in the sixth bar of the G flat or minor third of E flat, as distinguished from the major G natural, which might have been used, is instantly recognized." Again (p. 329.): "The effect of the minor intervals in this melody was noticed in the chapter on Harmony; but the character is really set in the opening six [five] bars of the piece, before any such interval has occurred. Here the motion, grave throughout, owes its character of absolute dejection mainly to the two groups of three descending thirds in the middle; these receive a special intensity from the accents falling on the second of the three chords; and the pause after the first group emphasizes the effect of the reiteration, as though the power of movement were gathered again only to sink to a still lower depth of depression. . . . . These features present a clear affinity to physical movements of drooping and collapse." Violin and piano.

Answers to VII.

A. It seems plaintive and supplicating to me, but not necessarily very sad; phrases *h* and *i* are perhaps sad.

B. The piece gives a *general* impression of *sadness*, rather than dejection; of mourning that the past is past, rather than of fear for the future or regret for the present.

C. The extreme dejection begins with the G flat, continuing through the next bar.

D. Phrases *e*, *f* and *h* contain the element of sadness; *i* is full of feeling; *i* and *a* and *g*, have the human feeling; the spiritual element comes in the *d* and *c*.

E. Phrases *c*, *f*, *h* (especially), pain! phrase *i* gives way.

F. All through, great dejection, especially *b c*, *e f*, and some notes in *h*; but (query) was it not the wail of the strings? Would it have been so on the piano alone?

G. Sad throughout, but especially in *a*, *d* and *g*.

H. To me the dejection is continuous.

I. The dejection comes first at *b*, next at *e*, but a trifle less here than at *b*. The climax is on *h*. Phrase *i* disappoints me as not fulfilling what was required to complete the dejection. The G flat in *h* is the decisive note.

J. The two falling cadences like sighs, and the last bar *giving way* to sadness.

K. The dejection comes in the first place from the abruptness produced by the short rests. The incompleteness of the short phrases *b c*, *e f*, suggests the cutting short of the life, whose grandeur is felt in the beautiful harmony in *a*, *d*, *g*, and *i*. These last form a connected whole of which the previous are an interruption.

In the second place the change of key in *e* and *h* adds to the sense of normal development interrupted; and this is further carried out by the minor in *h*.

The self-restraint produced by the pause between the second and third notes of the violin [which was played as above indicated, and as the phrase is sung, and not as it occurs in the introduction] adds to the pathos.

Phrases *a*, *d* and *g* represent the great soul struggling on its course; phrases *b c*, *e f*, *h*, represent circumstance, fate, the cutting off of the soul's course.

L. Close of phrases *a* and *g*, to some extent *b* and *c*, *d* and *e*. Most of all to me phrase *h* and beginning of phrase *i*, which seems to express an utter hopelessness. Of course, as a whole, the air is very sad.

M. Phrases *d* and *g* are spoken words of sadness; the phrases played by the piano alone are sighs; the halting notes at the conclusion are the culmination. The introduction of successive flats evidently causes a part of the effect.

N. The "dejection," apart from the tempo, is not marked in the first phrase, appears, however, in *b*, and in the contrast between the high and low phrases. The concluding phrase is one of a certain sorrowful consolation. Nor is the dejection anywhere without its accompanying and contrasted consolation, which appears in the phrases low in the scale as a certain gently solemn offset to the grief. In this contrast lies the chief art.

O. Decidedly at *h*. The rest is also sad, but *h* especially so; it is piercing. Dejection is not so much expressed in *h* as sadness.

A. Phrases *a* and *c*, deep sadness; *d* and *b*, tender compassion; phrase *h* is heroic; phrase *i*, exquisite pity (love).

B. The most touching, the three last measures.

C. Impression of dejection begins in *a*, increased in *d*, still more in *f*, complete in *i*; lost more or less, or perhaps I should say diminished, in the intermediate phrases.

D. The passage *d* expresses dejection, discouragement, most powerfully.

E. The whole selection contains profound dejection; the theme is announced in *a*; *b* and *c* are the sorrowful suggestions of the sufferer that there may be better things in store, yet even here the downward phrasing implies that hopelessness brings the hope immediately downward; *d* marks the better conclusion of the sufferer; *e* and *f* are new suggestions of hopeless hope, and *g* the heartbroken answer that the bitter fact remains; *h* is the admission of the counter-soul that pain is, after all, necessary; the final conclusion is one of the deepest depression.

F. Phrases *b* and *c*, and *e* and *f*, seem to express sadness, yet their expression of it would seem incomplete if they did not lead up to the phrases *d* and *g*; but *h* and *i* would, to me, separately express the tone of the whole fragment.

G. The poignancy of the sadness increases with each phrase, and culminates in the last heavy dejection and gloom at the end — a gloom not of anger, but of sorrow accepted.

H. The expression of deep sorrow and abandonment is accentuated at *e*, and, to my feeling, culminates in *h*, but after all, is very likely a resultant effect.

I. The phrase *a* is not conclusive, but taken to *d*, the expression of dejection is complete, without the other phrases.

J. Phrase *h*, beginning with the fifth note.

K. Impression of sadness through the whole piece.

QUESTION VIII.

What single adjective best expresses to your mind the general impression of the following music?

J. S. Bach. Well-tempered Clavichord: Prelude in E flat minor. In the "Conversation in Music" (p. 5.), Rubinstein writes: . . . . "the tragic in no opera sounds, or can sound, as it is heard in . . . . , or in the prelude in E flat minor of Bach's "Wohl-temperirte Clavier." Piano solo.

ANSWERS TO VIII.

A. Religious.

B. Unsatisfactory.

C. Tragically sad. Widow of a dead patriot.

D. Fanciful; full of fancy. Picture: Twilight; a woman playing and dreaming.

E. ? ? ? ? Non-significant.

F. ————————.

H. Sad.

I. Funereal.

J. Instability.

K. Not light enough for "*fantastic*"; too much matter for the merely negative "*disjointed.*" Whimsical.

L. Interesting, but to me not particularly beautiful or great. It seems incomplete, more like an introduction to something else.

M. Disjointed.

N. This kind of thing declines to be expressed except as, say, a seraph's song, a song of one excelling in knowledge.

O. Funeral march?

A. Satisfactory.

B. Soothing.

C. Heavily monotonous.

E. Known. Massive: the massiveness of a cathedral, with the delicate tracery of the frescoing and pillar ornamentation occasionally revealed by the light.

F. Gloomy.

G. Interesting and dignified; non-emotional.

H. Serious (philosophical), majestically elevated—but to a dizzy height à la Beethoven.

J. Languor; reluctance.

K. Contentment.

L. Feelings after a disappointment; not cheerless, but serious, and more uplifting than sad; at the same time more or less sad.

Question IX.

Can you connect the following melody with any marked type of personal character?

Mozart: Don Giovanni: Canzonetta in the second act, sung by Don Giovanni to a mandolin accompaniment, "Deh vieni alla finestra." It was assumed that there would be some, at least, in the audience who would not recognize the song. It seems to me to bring out the two fundamental characteristics of a Don Juan: his powers of passion and the mocking indifference that lies beneath. Gurney speaks (p. 469) of "the half gay, half tender gallantry of the Guitar Song in Don Giovanni." Attention was specially called to the contrast between the melody and the accompaniment. Violin and piano.

Answers to IX.

A. Known. Don Giovanni.

B. A conflict of emotions; on the whole the higher purpose triumphs over the baser.

C. Known; but it agrees with his character.

D. The violin melody, a child telling a story, or a story told to a child; the accompaniment suggests the lightness of the whole. It doesn't matter, after all.

E. D. J. and Laporello, nicht wahr? No other association, though I don't recall the last part.

F. Rollicking roué, the accompaniment; more depth of character in air.

H. Somewhat like the celebrated Don Juan Serenade; a passionate plea shut in in a laughing accompaniment.

I. Mountaineer; hunter. This was written before I noticed the air. Don Giovanni Serenade.

J. An earnest character with an underlying current of light-heartedness. One who thoroughly realizes the serious side of life, but who is by nature an optimist and cannot entirely repress a natural joyousness.

K. David Rizzio. A combination of a Watteau shepherdess and Priscilla Mullins. Marie Bashkirtseff.

L. A person of merry temperament, but at the some time of serious intent. The running accompaniment gives this merry irresponsible character to it. The air on the violin is more serious, though at times quite merry itself, and also occasionally sentimental.

M. A fashionable or frivolous character with a deep undercurrent of sincerity. This idea did not come until attention had been called to the difference between accompaniment and violin melody.

N. Beyond the impression of the tempo, nothing expressible.

O. Isn't this Don Giovanni's Serenade? The feeling is in the air, the villainy in the accompaniment.

A. Levity; amorous sentiment.

C. Frivolity, and the earnestness really in the character.

E. Known. This does not suggest one particular type of character, unless, perhaps, it is the double character of frivolity with a steadying strain of seriousness.

F. The contradictory character of a young woman.

G. A complex, fascinating woman, amid gay surroundings; or, a grave and thoughtful woman wearing a smiling exterior.

H. Known. Don Giovanni.

K. Known. Don Giovanni. Flippancy, or better, playfulness, feigned, concealing a sad heart.

L. Known. Don Giovanni.

QUESTION X.

Of what race should you think this song a product, and why?

"Der Rothe Sarafan." Russian popular melody. It has to my ear the monotonous sadness of the plains whence it has come. There is a terrible hopelessness in it; it seems an echo of immemorial misery borne with resignation.

Violin and piano.

ANSWERS TO X.

A. English.

B. The softness and the rather languid air about it betoken a southern nation, and I should say Persian.

C. Known; but I don't see why.

D. English.

E. Rothe Sarafan. Russian, I believe.

F. Irish; but too pathetic.

G. French.

H. German moonshine.

I. Old English; then recalled as Russian.

J. German. The music of a musical people, but of the peasantry because *simple*.

K. Central Germany.

L. Teutonic(?). Have no reason. Perhaps Slavonic(?).

M. English(?) or German.

N. Don't know it. Take it to be South German, but feel much doubt. Can feel no assurance.

O. Italian?

A. German — simplicity.

B. Not Italian, nor German, nor French.

C. A northern song, perhaps Norwegian or Russian, on account of its pathetic wildness.

E. German, because of its simplicity of feeling. Not light enough for France, nor sparkling enough for Italy, too much sentiment for England.

F. German.

G. Possibly Irish.

H. Known. Russian popular song. The melancholy is characteristic.

J. German. Sung by a homesick woman far away from her native land, possibly in America.

K. Unknown. Russian, because of its undercurrent of sadness.

QUESTION XI.

Is the singer of the following melody a man or a woman, and out of what emotional experience would such a song be born?

Bizet. The song of Carmen in the third act of the opera, over the cards that foretell her death: "In van per evitar." The dramatic situation for which this song was written seems to me to be expressed in the music alone; a woman sings thus to tell herself that though life is sweet she must shortly die.

ANSWERS TO XI.

A. A woman has lost her husband in battle and is trying to console herself while singing to her young babe. Mingled despair and love for her child.

C. Woman; disappointed love.

D. Might be sung by either, possibly woman. Suggestion, the spirit of Schumann's "Ich grolle nicht."

E. First hearing. Either a man pathetically and simply pleading to a woman to have him, or a woman kindly and sweetly saying she must refuse. Second hearing. Decidedly a woman, and decidedly pleading, with a longing yet submissive spirit.

F. Woman; a farewell.

G. A man pleading with a woman who does not reciprocate his affection.

H. Maternal love.

I. Woman; jealous love.

J. Woman; intense protestation, sad, not angry.

K. Male.

L. Woman. Expression of some deep passion.

M. A girl mourning for her dead lover.

N. A woman resigns her dearest—a mother her son, a girl her lover,—for a noble end, a cause that demands him; she does not resign him to a blind fate, but to a cause. (Not at all recognized.)

O. A woman's sorrow and prayer. It is a sort of sublime calm despair.

A. A woman. A very loving plea for reconciliation, explaining her loyalty and faith in suffering.

B. Man; disappointment and despair.

C. Man; yearning wistfulness.

E. A woman who has suffered sings because conquered pain impels her.

F. Man; sorrow.

G. A woman; bewailing past joys.

H. Is it written for a man's voice? perhaps approaching death.

J. Man; passionate entreaty without avail.

K. A passionate woman; unrequited affection.

L. Woman; bewails loss of her lover; first wretched, then despairing, finally experiencing a gentler feeling and determined to make the best of it.

The evening began with a performance of the Andante from Mendelssohn's well-known and loved Concerto for violin and piano (Op. 64), on which three of the auditors took the following notes:

C. First movements of an undeclared love; middle of same; some disturbing circumstances in same, followed by repose.

K. Yearning; longing to go; attainment seen but recognized as impossible. Passionate pleading—struggle to transcend limitations. The motive to the passion felt here are mixed. Partly they are the inevitable conditions of human life; partly the motive is love.

A. Education ethical; plea for purity and generosity; Strom der Welt; ethical practice on broad lines; justification; peace; beauty.

In ending the transcription of these notes of the first experimental attempt that as far as I know has ever been made to reach exacter notions in regard to the expressiveness of music, I cannot forbear acknowledging again my good fortune in obtaining aid of such temper and quality in the undertaking. Not only my thanks but those of all who are interested in the more careful study of the mental phenomena concerned in music hearing are due to this company of listeners for the zeal and the frankness displayed in these acute replies to an exacting series of questions.

Before proceeding to the examination of these suggestions from various music, let us endeavor to form some idea of the general nature of the inquiry in the pursuit of which they have been obtained.

To be continued.

PSYCHOLOGICAL LITERATURE.

PSYCHOLOGICAL NOTES.

By E. W. Scripture.

The Method of Regular Variation.

The method of minimum variation seeks to determine that change in the stimulus which produces a minimum change in the sensation. For this purpose the stimulus is varied in one of its properties till the variation is noticed. This is what the usual statement of the principle amounts to when we enlarge it from the exclusive application to the intensity of the stimulus. In applying this method to psychological questions it has up to the present day been changed in such a way that it can scarce be brought into agreement with the above view of it.

Given a stimulus possessing several properties x, y, z. Let one of these properties be varied, e. g. x, while the others are kept constant. We have thus two independent variables x and t, the varied property and the time. The problem is to find for what values of x and t the change becomes noticeable.

The expression for the variation of x is $\frac{dx}{dt}$. Now, if for $t = 0$,

$$v = x_0$$

and in general

$$\frac{dx}{dt} = u$$

where x_0 is a given constant (in this case the value of that property of the stimulus which is to be varied, or the "starting value"), and u is a given function of t, then x is determined for every value of t and the problem becomes: for a given value of the property x, which is varied at the rate u, to determine at what value of x the variation becomes perceptible. The value of x thus obtained is called the *least perceptible variation.*

The variation is likewise completely determined when for $t = 0$

$$x = x_0$$

$$\frac{dx}{dt} = u_0$$

and in general

$$\frac{d^2x}{dt^2} = U$$

where the quantities with the index $_0$ are given constants.

The quantity U as thus defined is called the acceleration of the variation. The value of x corresponding to the least perceptible change can be called *the least perceptible accelerated variation.*

In applying this method the property x is varied at the rate u, first increasing and then decreasing. This will give two values for the least perceptible variation, D_0 and D_u, from which we take

$$D = \frac{D_0 + D_u}{2} \text{ or } D = \sqrt{D_0 \, D_u}$$

The quantity D, however, is not a constant but a function of the rate of variation.

To find the least perceptible accelerated variation the intensity is varied, but at an accelerated (or retarded) rate. The results are treated in an analogous manner, and, denoting this quantity by A, we have

$$A = \frac{A_0 + A_u}{2} \text{ or } A = \sqrt{A_0 \, A_u}$$

A is a function of the acceleration U.

The starting value of x may, however, take on different values, x_0', x_0'', x_0''', ... Consequently the least perceptible variation and the least perceptible accelerated variation are not only functions of the rate of variation and acceleration but also of the starting value; thus,

$$D = f\,(u, x_0)$$

and

$$A = f\,(U, x_0).$$

Up to this point we have taken into consideration only one of the variable properties of the stimulus. In case it has more than one variable property, e. g., y, z, ... the least perceptible variation and acceleration of the property x may, through influences on the attention, etc., be dependent on the values of y, z, . . . If we distinguish the least perceptible variation and the least perceptible accelerated variation of the property x by the index $_x$, then

$$\begin{aligned} D_x &= f\,(u, x_0, y, z, \ldots .) \\ A_x &= f\,(U, x_0, y, z, \ldots .) \end{aligned}$$

Of course the least perceptible variations of y, z, ... are likewise

$$\begin{aligned} D_y &= f\,(u, y_0, x, z, \ldots .) \\ D_z &= f\,(u, z_0, x, y, \ldots .) \\ . \quad & \ldots\ldots\ldots\ldots \\ . \quad & \ldots\ldots\ldots\ldots \end{aligned}$$

and the least perceptible accelerated variations are

$$\begin{aligned} A_y &= f\,(U, y_0, x, z, \ldots .) \\ A_z &= f\,(U, z_0, x, y, \ldots .) \\ . \quad & \ldots\ldots\ldots\ldots \\ . \quad & \ldots\ldots\ldots\ldots \end{aligned}$$

If, with Wundt, we distinguish the minimum variation from the least perceptible variation, we get slightly different values for D_0, D_u, A_0 and A_u (see Wundt, Phys. Psy. I 350, Leipzig, 1887); in this case the term "minimum variation" and "minimum acceleration" are to be substituted for "least perceptible variation," etc.

In the applications of the method of minimum variation up to the present these quantities have been either neglected or treated in a manner not at all consistent with the method. The usual method or application can be illustrated as follows: Given the stimulus with the variable properties x, y, z, ... to find the least perceptible variation. One of the properties, x, is taken as variable, the others are made constants. Let the starting value be x_0. This starting value x_0 is given constant for an instant and then x is reduced to 0. This value of x,—that is, the absence of the stimulus,—is maintained for a time, n seconds; then another value of x is given, e. g. $x_0 + \Delta x_0$ and the person experimented upon is asked whether this value is perceived as different from the starting value of x. If not, x_0 is again given and after the same time, n seconds, another value of x, e. g. $x_0 + \Delta x_0$, is given and the same question is again asked.

This is an entirely different problem from the one proposed for solution. Here we have the question of a judgment of the likeness or difference between two stimuli given at two different times, or between a stimulus $x_0 + \Delta x_0$ and the *memory* of a stimulus x_0 which occurred n seconds before. The fact that we are able to judge with great accuracy this likeness between a stimulus and the memory of a stimulus given a short time before is not to the point; although that problem is one of vital interest also, it is not the same as the problem of the least perceptible variation of the stimulus x.

I have said that the quantities considered above have been neglected in previous investigations. I must, however, make at least one exception, namely, the original and weighty essay of Hall and Motora, *Dermal Sensitiveness to Gradual Pressure Changes*, AM. JOURNAL OF PSYCHOLOGY, 1888 I 72, to a study of which I owe the impulse to a development of the method of regular variation and to its application instead of the old form of the method of minimum variation. This is, as far as my knowledge goes, the first time the method has been applied to psychological questions.

It is my intention to apply this method to a number of the problems of psychology. Two such investigations have already been begun, one on the faintest perceptible sound, the other one on the least perceptible variation of the pitch of tones; some of the qualitative results are given in the following notes. The apparatus in preparation will admit without any change the investigation of the problems of the least perceptible variation of intensity of pitch and of noises, of the lowest perceptible tones with certain (not all) degrees of intensity, and several others of which indications have shown themselves. A slight change will enable the solution of problems concerning the least perceptible accelerated variation; indeed, the difficulty of obtaining a constant rate of variation instead of an acceleration is what first called my attention to the subject of the perception of accelerated variation.

ON THE LEAST PERCEPTIBLE VARIATION OF PITCH.

Continuous variations of pitch are best obtained from the wave-siren. In the wave-siren a constant blast of air is directed against the edge of a rotating disc which is cut into waves so that a variation of the current of air is produced which follows the law

$$y = a \ \sin \mu t$$

where y is the change in the current at the moment t, a is the extreme value of y and μ is the vibration-index. This gives a simple tone, the pitch of which depends on the rate of rotation.

One of the chief purposes for which the wave-siren has been set up in Clark University is the investigation of the sensitiveness to variations of pitch according to the method of regular variation. The researches of Delezenne,[1] Seebeck,[2] Preyer[3] and Luft[4] have been made with continually increasing accuracy, but all of them have left out of consideration the important element of time. The influence of this factor is taken into consideration and measured by employing the improved form of the method of minimum variation, namely, the method of regular variation. The arrangements for making the measurements are nearly complete; in the mean time the qualitative result has already shown itself.

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[1] DELEZENNE, Mémoires sur les valeurs numeriques des notes de la Gamme, Recueil des travaux de la soc. de Lille, 1826-27, p. 4.

[2] SEEBECK, Beiträge zur Physiologie des Gehör- u. Gesichtsinnes; C. Ueber die Fähigkeit d. Gehörs, sehr kleine Unterschiede der Tonhöhe zu erkennen, Pogg. Ann., 1846 LXVIII 462.

[3] PREYER, Grenzen der Tonwahrnehmung, Jena, 1876.

[4] LUFT, Ueber die Unterschiedsempfindlichkeit für Tonhöhen, Philos. Stud., IV 514.

As was to be expected, the least perceptible variation in the pitch of a simple tone is not only a function of the pitch and the intensity but also of the rate of variation. Given a tone of the pitch n and the intensity I where $I =$ constant, to find the just perceptible Δn when the pitch of the tone is varied at the rate $q = \frac{\Delta_n}{t}$. The preliminary experiments already prove that

$$D \propto \frac{1}{q}$$

That is to say, the least perceptible variation increases as the rate of variation decreases, and *vice versa.*

On the Faintest Perceptible Sound.

By means of a specially constructed audiometer I am able to vary the intensity of any given sound at almost any given rate. While preparing for an extensive series of measurements on the least perceptible sound I came across the following remarkable fact.

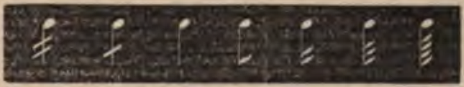
The audiometer is arranged so that the sound is heard in a telephone, and its intensity depends on the relative positions of the primary and secondary coils. At one point there is an absolute zero, that is, the plate of the telephone is not set in vibration. As the position of one of the coils is changed a vibration is set up in the plate, the amplitude of which depends on the strength of the current. If we change the position of the coil sufficiently the vibration of the plate and the air become great enough for the ear to perceive a sound. The point at which this happens — that is, the faintest perceptible sound — depends on the sensitiveness of the person tested and on several other factors, one of which has hitherto been left unnoticed, namely, the rate at which the threshhold is approached. The influence of the rate manifests itself in a peculiar manner. If the relative position of the coils is very slowly changed the faintest perceptible sound appears at a certain point where the physical vibration has a certain amplitude, which we can denote by p. Thus, when the amplitude of the vibration has been changed from 0 at the rate of q units per second, then the faintest sound perceived is p. But a change in the rate q, according to which the sound is varied from zero, produces a change in the faintest perceptible sound p, making it p'. That is, a change in the rate at which we pass from an absolute zero to the psychological zero causes a change in the relation between the two.

The apparatus for the absolute measurement of sound intensities in units of work has not yet been completed, so I can not give the exact law governing the relation of these quantities; but from the experiments as yet made one fact at least can be determined: the least perceptible sound has a greater physical value as the rate is increased. Judging from rough estimates I should say that this physical value varies as the square of the rate of change.

Notation for Intensity.

Although the modulations of intensity have been carried to a great degree of fineness in music, the amount of intensity in any given case has been left to the semi-conscious judgment of the musician and has not, like the questions of pitch and duration, been reduced to any regularly defined principles. In the history of music the first of the three dimensions of tones to push itself into conscious recognition was that of pitch; thereafter followed duration, and last intensity. The first complete notation for pitch is attributed to Guido Aretius in the XI. century. Three centuries later the notation for duration was introduced by Jean de Meurs. Naturally the presence of exact means of expression for these two quantities afforded opportunity for development in the artistic execution on the one hand and for scientific research on the other. The subject of pitch has reached a high degree of development. The duration of tones is also a matter of technique that has been carried to a great degree of precision in practice, although it has been scarcely investigated scientifically. The intensity of tones, however, has been much neglected; it must be remembered that we are not speaking of the semi-conscious use of the different degrees of intensity in the execution or composition of a piece of music, but to a deliberate use of the shades of intensity. In music the consideration is confined to the five vague expressions, *ff*, *f*, *m*, *p*, *pp*. In science the question has been treated almost exclusively in relation to the rough tests for deafness. The late attempts to find a measure of the intensity of sound have come more from the efforts of physicists to satisfy the needs of the specialists for ear troubles than from any advance in the music of intensity.

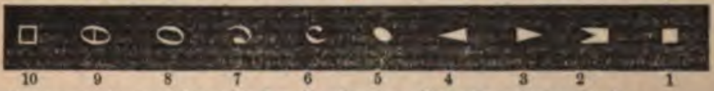
Before much advance can be made, it will be needful to adopt some expressions for the different degrees of intensity to remedy the inconvenience and inaccuracy of the present terms. For the present purposes we can make use of a system limited to ten degrees of intensity. Pitch is indicated by the position of the note on the staff, duration is shown by the hooks on the stems of the notes, except in the case of the whole and half notes, where a difference is made in the head of the note. This change in the head of the note is unnecessary for the indication of duration and can be employed to indicate intensity. A very slight change is thus necessary in the present notation; we can retain the usual method of indicating pitch and the usual signs for duration with the exception of the two for the whole note and the half note. These can be indicated by two lines across the stem of the ordinary quarter note for the whole note and one for the half note. Consequently the series of notes as regards duration will be



representing the whole, half, quarter, eighth, sixteenth, thirty-secondth and sixty-fourth notes respectively.

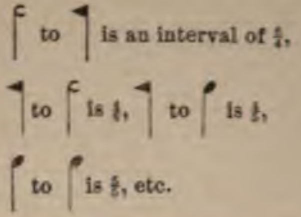
Whenever it is desired to write music without regard to intensity, it can be done in the same way as at present with the substitution of the two new signs for the whole and the half note, or it can be done as usual without any danger of there being a mistake in the playing of it. Moreover the comprehension and the execution of pieces in the usual style will not be in the least interfered with.

The heads of the notes in the above example are all the same; by employing different kinds of heads the different degrees of intensity can be indicated without introducing new complications in the notation. The degrees can be indicated by a series of new signs. Ten different degrees of intensity are to be represented; they can be said to stand in the relations expressed by the numbers placed under them in following list:



These can be used as the heads of notes directly on the staff.

The question arises of how to name intervals of intensity. To avoid the confusion, inaccuracy, and above all the limitedness of the names used for intervals of pitch, we can adopt a simple system in regard to intensity. An interval of intensity is to be designated by a fraction of which the degree of the denominator is the degree of the first of the two tones, and the numerator that of the second. For example,


to is an interval of $\frac{6}{4}$,
to is $\frac{4}{6}$, to is $\frac{5}{6}$,
to is $\frac{5}{5}$, etc.

All of the degrees used at present can be indicated as well as several more. The worth of the notation does not, however, lie in this, but in the fact that these degrees and distinctions can be introduced into the notes on the staff, thus making it possible to manipulate the shades of intensity in music just as is done with pitch and duration.

A CONSTANT BLAST FOR ACOUSTICAL PURPOSES.

Töpler and Boltzmann in their essay, "*Ueber eine neue optische Methode, die Schwingungen tönender Luftsäulen zu analysiren*," Pogg. Ann., 1870 CXLI 341, make the complaint that it is impossible to obtain a constant pressure of air for blowing acoustical instruments, in spite of the regulators that are applied. They suggest the possibility of using some kind of rotary blower. In making some experiments with organ pipes a while ago, I was forcibly impressed with the same difficulty, although I had access to the best kind of bellows made, namely, that of Cavallé-Col as made by König. At a later date, when contemplating some experiments on the sensitiveness to variation in pitch, I came to the conclusion that unless a far greater degree of constancy in intensity could be obtained, any experiments on the sensitiveness to variation, either in pitch or in intensity, could not be made with the degree of accuracy necessary in psychological experiments. The first step was therefore to find a constant blast. After considerable time spent in fruitless trials I have adopted an arrangement which has proved quite successful.

The chief requisite is a rotary blower giving a sufficient pressure. The blower giving the greatest pressure with the least expenditure of power is the Root direct pressure blower. This, however, is inapplicable for acoustical purposes, owing to the vibrations of the air produced by the cams, which make a deafening noise when the blower is run rapidly. The difficulty is present to a far less degree in the Sturtevant blower. This blower employs a blast wheel of a special pattern, so arranged as to produce a movement of the air from the center to the periphery. Owing to a careful construction of the fans the pressure on blast thus produced has a constancy beyond any means of measurement at my disposal. Moreover, the adjustment of the center of gravity is so good that the wheel can be run at a high speed without danger. There are two disadvantages about the blower, namely, the high speed required to produce sufficient pressure and the production of tones in the blower when going at such a high speed. By courtesy of Prof. Michelson of the physical department of Clark University, I was allowed to run the belt of the blower directly from the drive-wheel of the engine, the drive-wheel being 31 inches in diameter and going at 340 revolutions per minute. The blower used is numbered 0000, stands 12 inches high, has an outlet of 2½ inches, and a pulley of 1⅞ inch diameter. Driven by this engine the wheel makes 5600 revolutions per minute.

To get away from the noise of the machinery and the tones produced by the blower, a 1½ inch pipe is run to another room, where the sound cannot be heard. The sound brought through the pipe is so faint that it cannot be heard unless the ear is placed at the opening; it thus does not interfere with the use of the blast in any experiments yet tried. A specially made fan with curved blades is said to still further reduce the sound in some blowers lately made. The slightness of the sound produced by the blower is shown by the fact that in running the wave-siren from the lowest pitch up to the highest obtainable, no beats appear, whereas, an attempt to sing the tone of the siren near the rubber tube connecting the mouth-piece with the main pipe at once causes loud beats.

With the wheel running at the above rate and at a distance of 65 feet from the blower, a blast of 10 to 11 centimeters (4 in.) water is obtained. An engine such as here used is, of course, unnecessary. To supply a blast of 12 centimeters pressure with a discharge of 60 cubic feet per minute would require a speed of about 6000 revolutions per minute for the blower wheel and a motor of less than ¼ horse-power. If the motor has a large drive wheel the belt can be run directly from it and no counter-shaft is required.

The great importance of the arrangement here described lies in the greater degree of accuracy thus introduced into psychological experiments on hearing due to the possibility of maintaining a constant intensity.

Some Psychological Terms.

It is the purpose of the present remarks to propose short definitions for a few of the fundamental psychological terms. These definitions are not, however, to be considered as fixed; unchanging definitions are a sign of decrepitude. They ought to progress with the advance of the science; but at each stage of development it is absolutely necessary that certain names should be appropriated to certain things and that everybody should clearly understand just what they are appropriated to.

One of the most abused and indefinite words is "sensation." Bain uses the word to designate the mental impressions resulting from the action of external things on the body. Others take for granted a whole theory of the relation between the mind and the nervous system; Volkmann calls a sensation "the condition which is developed by the soul on the occasion of a nerve-stimulus that is brought to it." Sully defines it as an elementary mental phenomenon that cannot be defined in terms of anything more simple, its meaning being capable of indication only by a reference to the nervous processes on which it is known to depend; as almost absolutely nothing is known concerning these nervous processes, it would seem that the meaning of sensation cannot even be indicated. There is another class of definitions of which this from Carpenter can serve as a specimen: "Sensation is that primary change in the condition of the conscious *ego* which results from some change in the *non-ego* or external world." The easiest way is to shirk the duty of defining the word at all, as only too often occurs. From the lack of acquaintance of the principles of defining which is shown in those that undertake the task, the hesitancy of those who do not is readily understood.

And yet it ought not to be so difficult to agree on some definition. A term is a word used to represent a group of phenomena, a definition is a statement of the phenomena denoted by the term. To avoid the impossibility of going over all the phenomena denoted by the term, we c give a definition by stating what properties a phenomenon must ha order to be denoted by the term, what properties it may not ha what properties are indifferent. Usually it is sufficient to give the essential properties, omitting the excluding and indifferent ones.

The crying necessity in psychology is a term that shall denote the simplest mental phenomenon, just as "atom" denotes the as yet undivided particles of matter. As such a term we propose the word feeling. Feelings would then be defined as the indivisible elements of mental phenomena; they are as yet unanalyzable components. Nothing is said of the relation to an external or an internal world; no hypothesis is introduced as the nature of mind; all that is assumed are the existence of certain phenomena, called mental phenomena, and the possibility of analyzing them. The ultimate results of the analysis may be what they will, we always have a name for them; with perfection of methods and apparatus the analysis will be pushed further so that what is to-day regarded as a feeling may prove to be a compound, nevertheless the term has always the same meaning.

The proposed definition is by no means entirely new; in fact the word most frequently used to denote simple mental phenomena of all kinds is probably this very term. It is used in almost this way by Spencer. "Each feeling . . . . . is any portion of consciousness which occupies a place sufficiently large to give it a perceivable individuality;" the definition proposed differs from his in not setting apart a special class of "relations between feelings."

Among feelings we find innumerable kinds. When we attempt to class them according to their likeness to one another, we find several ways of arranging them. The most usual way is to class them into two lots, according to whether we regard them as passively experienced or actively experienced. Here is just where we find the thought that is at the bottom of all the array of definitions of sensation; when stripped of theories we can well agree to appropriate "sensation" to the passively experienced feelings. In a like manner "impulse" can be used to designate the actively experienced feelings. These mental elements can, of course, be further classified into sensations of light, of sound, impulses to action, to inhibition, etc.

Terms for the indivisible elements having been obtained, it becomes necessary to have names for the compounds. To designate a psychological compound in general I venture to propose that much abused and at present indefinite word "idea." Possibly this may be justified by referring to an almost similar use of the word by Descartes (See Eucken, Grundbegriffe der Gegenwart, Leipzig, 1878, 225).

Ideas, however, seem to fall naturally into two great classes, according to the preponderance of sensations or impulses in their composition. The former class can be called "percepts" with scarce a departure from such a portion of definite meaning as the name now has. In a corresponding fashion the ideas composed mainly of impulses can be called "volitions." Compounds of higher grades can of course receive appropriate names.

The following list of terms is proposed for psychological use according to the definitions attached to them:

1. Feelings are the indivisible elements of which mental phenomena are composed. Every fact of consciousness that has not been proved to be a combination of other facts is to be called a feeling.
2. Sensations are those feelings that are regarded as coming from without; they are passively experienced feelings.
3. Impulses are those feelings that are regarded as originated in the mind itself; they are actively experienced feelings.
4. Ideas are compounds of feelings of any kind; they are the psychical units.
5. Percepts are those ideas that are composed mainly of sensations.
6. Volitions are those ideas that are composed mainly of impulses.

FACILITIES IN EXPERIMENTAL PSYCHOLOGY AT THE VARIOUS GERMAN UNIVERSITIES.

By William O. Krohn, Ph. D.

The writer returned in March from a sojourn of nine months on the continent. From two weeks to three months were spent at each of the principal German university centers. The time was occupied in a study of men, methods and laboratory equipment at those universities enjoying facilities for work in experimental psychology. Many teachers who contemplate starting laboratories in this country have suggested that we give information in the form of a printed article as to the apparatus used, its cost and value. We have already done this in several instances through the ordinary channel of private correspondence. The present article concerns apparatus only. We hope later to give a sort of pen picture of the men who are now contemporaries in furnishing the instruction in experimental psychology in the several universities of Europe which the writer has visited as well as to present the results of careful and rather extended study of their methods of work. The time was spent at Heidelberg, Strassburg, Zurich, Freiburg, Munich, Prag, Berlin, Leipzig, Halle, Göttingen and Bonn, in the order named. The longest periods were those at Freiburg and Berlin. At each of these universities the laboratories were carefully inspected and in some of them the writer carried on experimental work. In our description, for sake of convenience, we will follow the order given above. The description of the excellent laboratory at Göttingen, however, much to our regret, must be omitted from the present article.[1]

HEIDELBERG.

Heidelberg was visited in order to learn from Kuno Fischer and Caspari the real trend of psychology and philosophy at the present time, especially to ascertain to what extent the English Associationalists, and more particularly Herbert Spencer, had influenced German thought. Fischer is openly opposed to all experimental research in psychology. He regards it as a temporary fad, a side issue, an illegitimate method, lying wholly outside the main stream in the development of psychology as a science. Caspari is much more friendly, and reads lectures on experimental psychology, but does no laboratory work, and has had no laboratory training. To the medical faculty belongs the credit of the projected laboratory at Heidelberg. Prof. Kraepelin lectured on "Physiological Psychology" and succeeded in attracting and interesting enough men to warrant him in offering in a modest way a course of laboratory instruction. His equipment is small and he fails to distinguish between psychological and physiological experiments. Naturally the latter predominate.

STRASSBURG.

At Strassburg there is no psychological laboratory, but Goltz and his assistant, Ewald, have much to show that is full of interest. Goltz has a large collection of animals — dogs, monkeys, pigeons, salamanders, etc.— from which the cerebrum has been wholly or partially extirpated. Nowhere is vivisection carried on on a larger scale than at Strassburg. Indirectly there is much valuable material to be made use of at this university. Ewald is ingenious as a mechanical contriver. His newly invented chronoscope has certain advantages over the Hipp machine and is much cheaper (95 marks). It is described at great length and illustrated by two cuts in a thesis by Otto Dumreicher.[1] The larger size is the more desirable. This little instrument is, in fact, only a counting apparatus which records the number of breaks in an electrical current which passes through the instrument. A tuning-fork that makes a hundred vibrations in a second is a very convenient and simple instrument for the breaking of the current into a number corresponding to the number of vibrations. Thus one obtains the time to be measured expressed in hundredths of a second. The chronoscope then merely counts the number of these breaks in the electrical current, i. e., the number of vibrations made by the tuning-fork. For the purpose of counting these breaks there is in the chronoscope a little electro-magnet which moves a little ratchet in and out. This ratchet or trigger in turn moves a little toothed wheel, which possesses a hundred teeth, and on this wheel is an indicator. In this way the movements of the electro-magnet which is attached to the ratchet are transmitted to the indicator so that a spring or weight is unnecessary. This is one of the most pleasing features of the apparatus, since no winding of clock-work is needed. It is so arranged that the toothed wheel can not be moved backwards, neither can a single tooth be skipped. The instrument can never make a false record. It remains only to speak of the way in which the indicator is brought back to zero. The ordinary complicated arrangement of a stop-watch would not serve the purpose of Prof. Ewald. He has solved the problem in the simplest possible manner. The entire inner part of the chronoscope (electro-magnet, toothed wheel, and indicator) turns upon an axis, while the dial plate over which the indicator moves is fixed and immovable. After each single test one can very readily turn the whole thing, indicator and all, back to zero on the dial plate. The key used by Ewald in connection with his chronoscope is equally ingenious. It consists of a double armed lever so arranged that the current is closed when one presses on the button, but opened as soon as one withdraws the finger. It costs 20 marks, and is illustrated in Dumreicher's paper as well as Majer's catalogue. Ewald's mechanic, Majer, also makes an excellent "inductorium," modelled after that of Du Bois-Reymond, but somewhat improved. It costs from 80 to 100 marks. His pseudoscope should be in every laboratory; it costs but 25 marks. Ewald has also invented a color mixer, which makes it possible to vary the sectors while in motion. It is not only very complicated and expensive (200 marks), but also very unsatisfactory as well. It doesn't always work and is frequently out of order.

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[1] The material collected for the portion of the article on the laboratory at Göttingen was loaned some weeks ago to a fellow-worker, who wished to order some of the excellent apparatus contained in this list, and has not been returned as yet The writer makes apology to Professor Müller and Dr. Schumann, and promises to give their laboratory the full treatment it merits, in the next number of the Journal.

Wendelband in his lectures pays some little attention to experimental psychology.

ZURICH.

Zurich attracts the psychologist mainly through Dr. Forel. His work is chiefly in the line of psychiatry. In his "Anstalt" one sees much interesting pathological brain matter — indeed the best at any of the German universities. Work in neurology and histology is carried on continually in a well appointed laboratory, which is under the immediate supervision of Dr. Forel. The writer is greatly indebted to this delightful man for showing him many hypnotic experiments. Hypnotism is constantly employed by Dr. Forel in this Anstalt as a means of cure. The clinics in psychiatry supply much interesting material for study and examination. Prof. Avenarius lectures on general psychology, but contributes nothing on the experimental side.

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[1] Zur Messung dei Reactionszeit; Strassburg, 1889.

FREIBURG.

The laboratory of Dr. Münsterberg is distinctively a *psychological* laboratory. A distinction is constantly made by him between psychological and physiological experiments. His laboratory is on a private foundation established by Prof. M. himself, out of his own resources. To use his own figure, "The university authorities appropriate scarcely enough to buy the little bit of quicksilver used." The laboratory has little more than a nominal connection with the university of Freiburg, and is located in two large rooms of his private house. The students work in the laboratory from 11 to 1 each day of the week, though it is open at all hours to such as wish to carry on research work in connection with special problems. His courses are "privatim" but "unentgeltlich." The laboratory is provided by the professor with all the current literature. His apparatus is all practical, designed by himself, and constructed by his mechanic, Elbs. Aside from the staple apparatus to be found in every laboratory (Hipp chronoscope, meteronomes, etc.), the following pieces are of especial interest and to be commended for their real value:

1. Muskelsinnapparat. This is the apparatus used by Delabarre in gathering material for his thesis which earned him his doctor's degree. In his paper he has an excellent cut of this apparatus.[1] This apparatus is made for the right or left arm. It is better to have two pieces of this apparatus so as to be able to make bi-lateral movements of the arms for comparison. The following is a brief description: Into a heavy iron stand moves a strong iron rod a little over an inch in thickness, which can be easily elevated or lowered at will, and by means of a screw can be held any height, something on the principle of a music stand or modern piano lamp. Above on this stand, arranged to turn about an axis, is the portion of the apparatus which is more immediately concerned in the experiment. This consists of two small rails, upon which runs a light easily moved car with four small brass wheels. In order to hold the car fast upon the track at any chosen position or angle, there extends out from the under side a piece of metal, which has upon the end a small wheel, that runs along the under side of a third rail. This third rail is midway between and a little above the other two. To the car is attached an indicator, the point of which indicates the position of the car upon a scale 900 mm. long. This scale is fastened upon the side of one of the outer rails and divided into half centimeters; millimeters can easily be measured with the eye. To the top of the car is attached a short hollow brass cylinder, into which the end-joint of the index finger can be placed in order to set the car in motion. Upon the middle track are two clamps, which in every chosen position can be fixed as limits for the movements of the car, if such limits are desired. At each end of the middle track are little pulley wheels, over which a string can be drawn, which is fastened at one end to the little car and at the other to a scale pan, which is used in experiments where the weight of the wagon is to be compensated or where the movement of the car by means of a weight is to be made more difficult or easier. The portion of the apparatus consisting of the rails and car is movable about an axis and can be made to stand at any angle between the horizontal and vertical position. Thus the car can be moved horizontally or vertically, or at any incline. By means of the compensating weights in the scale pan, the resistance of the moving car is almost entirely done away with. The apparatus is well made and lends itself to a multitude of conditions, and is exceedingly useful for many purposes. Price 130 marks.

2. A very convenient chronoscope, incased in wood so as to lessen the noise. It measures hundredths of a second very accurately and is

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[1] Ueber Bewegungsempfindungen. Pub. by Epstein in Freiburg, i. B., 1891.

very useful in ordinary reaction-time work. It runs twenty minutes without re-winding. It is not electrical, but runs by springs. It is much more convenient than the Hipp chronoscope. It costs 200 marks.

3. Augenmassapparat. This consists of a wooden board about 600mm. long and 500 mm. high, standing upright on its side edge. It is covered with very dark green felt, since this color is less fatiguing to the eye. At the back of the board are two scales, one vertical, the other horizontal. These scales are sliding and divided into millimeters. By turning a screw, the scale, either vertical or horizontal, may be moved, and in moving the scale the two white points on the front of the board are moved nearer to each other or away from each other at will. The exact distance can be read in millimeters by referring to scale on the back. The subject is made to judge distance moved as compared to a certain norm or standard given at the beginning of each test. Thus the method of average error is the one employed. Münsterberg has made over 30,000 tests with this piece of apparatus.

4. A key-board for reaction-time determinations, consisting of 50 buttons in electrical connection. These buttons can be labeled to suit the experimenter. Price 19 marks.

5. Schall-apparat. This apparatus is constructed for the purpose of determining the direction of sound. It consists of a very small box containing electrical apparatus, that makes the ticking noise, which acts as auditory stimulus. This is enclosed in a large oak box, deadened by a lineing of felt and zinc, and the space between the two boxes filled with water. The electrical apparatus can be set going from without by means of a small Grove cell battery. Through one side of the outer box are inserted 3 iron tubes that enter the inner box, and through which the sound can be transmitted. To these are attached rubber tubes about four feet long, at the end of which are bell-shaped gutta-percha cups that fit over the ear; these can be held at any angle, and thus the sound made to come from any direction. The subject then makes his judgment as to the direction in each case. It is a very convenient and ingenious device, and cannot be gotten out of order. Price 60 marks.

6. Fall-apparat, costing 20 marks.

7. Sphygmograph, 250 marks.

8. Color-mixers which are run by electricity are also to be recommended for their simplicity of construction and their general satisfactoriness. The laboratory also contains a "Ton-messer" of large compass constructed by Appum of Hannau, a / m.

A word ought be said with reference to Münsterberg's method of instruction. He sets apart each day for some specific class of experiments. Thus, Monday might be the "color-day," in which the students have the advantage of seeing all the apparatus connected with color-work. Tuesday would be the "sound-day," in which the experiments concerned sound alone. Wednesday would perhaps be devoted to psychometric methods, and so on through the week. After several weeks of this sort of introductory work, the students who are inclined select special problems for research, and others assist the professor in carrying on his own investigations. During the past year the professor has made many thousands of experiments that bear upon the general subject of daily rhythm. Some of this material will be treated in his paper at the congress to convene in London this summer. Professor Flournoy, in establishing his laboratory at Geneva, ordered duplicates of all of Münsterberg's apparatus.

Mention must also be made of Prof. von Kries, who is located at Freiburg with one of the best equipped physiological laboratories in the world. It is in a large, brilliantly appointed building, recently constructed, and well supported in a financial way by the government of

Baden. The lectures by this professor on the "Physiologie der Bewegung u. Empfindung" are replete with interest for the psychological student as well as are some of his practice courses in the laboratory under his immediate direction.

MUNICH.

At Munich there is no university laboratory in experimental psychology. But this does not imply that the students in this university are entirely without facilities in this line, for Professor C. Stumpf has a very useful though unpretentious collection of apparatus for sound experiments accumulated by him in order to carry on that large amount of experimental work in this field, in which he is recognized as an authority, and in which he has labored so assiduously. His methods are of the most painstaking sort, and "exactness" is a fitting label for all his experimental work. He works the same problem over and over again regardless of time involved, subjects all his results to a revision in which the most exacting tests are used, confirming and reconfirming his previous conclusions, and all this time aware that the world is awaiting his long promised third volume on "Ton-psychologie." He has an especially constructed "Ton-messer" (Appum), and the best series of forks found anywhere, which were taken from a piano constructed entirely of tuning forks. He uses much other apparatus along the other lines of psychology to illustrate his lectures in the class room.

The writer is indebted to Dr. Freiherr von Schrenck-Notzing, who showed him much in the way of hypnotic experiments. This well known physician constantly makes use of hypnotism as a therapeutic agent. He is also a close student of psychology, as Stumpf, Edward von Hartmann and others testify. Schmidkunz, a young man, lectures at the university on hypnotism once a week. Stumpf's lectures on applied psychology or pedagogics constitute an interesting and valuable course.

PRAG.

In the German university at Prag, Prof. Marty has a small collection of apparatus for purpose of illustration. It is on a very small scale, and no research work is done in psychology. Professor Hering's work in connection with the physiology of the end-organs of sense makes his laboratory an interesting place for the psychologist. The laboratory or institute consists of ten very large and well appointed rooms; the professor has three assistants. His mechanic, Herr Rothe, will furnish catalogue of instruments on application. Some of the apparatus for color work is absolutely indispensable to the well equipped psychological laboratory.

It might also be mentioned in passing that Herr Fric is located at Prag, from whom excellent models, charts and prepared tissue can be secured.

BERLIN.

Professor Ebbinghaus is at the head of the work in experimental psychology at the Berlin University. The authorities have set aside two rooms for his use. He has but little apparatus—Hipp chronoscope, tuning forks, brain models, meteronomes, and in general such pieces as are found in every laboratory. Special mention must be made of a color-mixer so designed that one can change the sectors without stopping the machine. It is very ingeniously conceived, but poorly made by the mechanic. Professor Ebbinghaus is exceedingly clear in his lectures, which are well attended, and he deserves from the powers that be, better support in a financial way to carry on the work in experimental psychology, work which he has well begun and for which he is so admirably fitted. It is greatly to be regretted that Germany's largest university is not better equipped with apparatus.

Munk's work at Berlin is very similar to that of Goltz at Strassburg. His lectures should be attended by all students in psychology who have the opportunity of studying at Berlin. His work in extirpation is always of profound interest. One of his students, Dr. Max Dessoir, who has recently distinguished himself by an elaborate series of interesting and important experiments upon the sensations of the skin, has just been made docent in the university. He proposes to extend the study to all the other senses as rapidly as he is able to do so. His work in connection with the temperature sense was carefully observed by the writer of this paper, with interest and profit. This young investigator has through his recent publications put some new and valuable material into the hands of psychologists. His lectures are upon "Psychological Basis of Æsthetics." The student at Berlin has access to Goldscheider, Preyer, Du Bois-Reymond and Helmholtz, besides the advantage of hearing Zeller, Lazarus, Dilthy, Döring and Paulsen. There is in Berlin a society composed of men interested in experimental psychology, which holds meetings every two weeks, at which a paper embodying the results of original work is read by some member or invited guest. These papers are afterwards published in the society's "Transactions." Dr. Dessoir is secretary of the Berlin society, and Dr. von Schrenck-Notzing of a similar one at Munich. They partake in some degree of the nature of the American Society for Psychical Research. Dr. Moll, the hypnotist, who is no less a psychologist, is a member of the Berlin society.

LEIPZIG.

Professor Wundt's laboratory is so well known, and his apparatus has found way into so many places, that perhaps it needs no detailed description, and yet the writer feels justified in giving a complete list in the hope that it will be of some assistance to those who are projecting laboratories — the persons for whom this article is chiefly written. Some of the apparatus is a little antiquated and has been superseded. Still, even this is useful for the purpose of illustration and verification. Professor Wundt has two able assistants in Dr. Külpe and Dr. Kirschmann. The laboratory or "institute," as it is officially styled, embraces six rooms conveniently arranged and tolerably well lighted, but with poor floors. Wundt contemplates moving to other quarters. The apparatus consists of one new Hipp chronoscope, price 282 marks. One Kugelfallapparat to test chronoscope; this is of little value and now seldom used, 64 marks. One large control hammer, which occupies 7 tenths of a second in falling; this is for the purpose of regulating the chronoscope, is finely constructed and costs 275 marks. It is accurately described in the last number of the Phil. Studien (Vol. VIII., page 145 ff.) by Külpe and Kirschmann. Cuts of the instrument are shown in connection with this article. One rheochord, 15 marks; one Fallapparat with a slit in the plate displaying the letter or word, 125 marks; one Fall-chronometer with automatic contact (Cattell), 145 marks. This is described in the Phil. Studien, Vol. III., pp. 307 ff., and is a very useful piece of apparatus. One "Sprech contact-apparat," in which the current is made or broken by speaking into a membrane-covered drum, costs 33 marks; one Schall hammer with electrical connections, so that it strikes when current is made or broken; meteronomes with and without bells, 12 and 15 marks respectively; one adjustable electro-magnet with stative, 9 marks; color mixers, run by clock-work, 54 marks each; the same with governors attached, 64 marks; one electric chronographic tuning fork of 250 vibrations, 90 marks; the same

(Helmholtz) with 125 vibrations, 75 marks; tuning forks mounted on resonant boxes, cost according to size; one reaction apparatus of ten buttons in electrical connection (Merkel), 56 marks. One large, heavy pendulum with horizontal and vertical slits in the bob. The subject, looking through tube as the pendulum passes, discerns the letter or word back of the pendulum, as it is displayed through the slits when the pendulum oscillates to and fro. Two adjustable magnets, reaction-time studies of light impressions, 275 marks. One chronograph for the measuring of very short time-intervals, consisting of large drum revolved rapidly by means of heavy weight. There is a cut of this apparatus in Wundt's Phil. Studien, Vol. IV., p. 458. It is extremely expensive, costing 700 marks; the same results can be obtained by a revolving drum propelled by a little three-dollar electric motor. One Zeitsinnapparat with six contact keys. This consists of a vertical drum revolving within a circle on which is a graduated scale. To this circle can be attached six contacts at different distances, the distances read in degrees, minutes and seconds by referring to the scale. A point on the drum in revolving touches these separate contacts in turn, thus making a current at each contact, which is communicated to an electric bell or hammer. The subject then judges and compares the time intervals between the sounds; costs without drum 124 marks. It runs by weights. It is described by Ester in the “Studien,” page 38 of volume II. With the large drum and recording apparatus it costs 350 marks. One large Fall-apparat with 4 electro-magnetic ball-holders. This is the piece of apparatus used by Professor Angell of Stanford University when at Leipzig in determining, according to the method of average error, the estimation of various intensities of sound. The apparatus consists of 4 upright polished iron or nickel rods, on each one of which is a little holder for the small ball. In making the current these holders drop the balls upon a block of ebony. As the balls are elevated more and more the sound becomes more intense. With four of these upright standards four successive sounds of different intensities can be obtained without re-adjustment. This piece of apparatus is more fully described in the “Studien,” Vol. VII., p. 425. One 3-fold diaphragm with an arrangement whereby the square holes can be readily adjusted, by means of a micrometer screw, to various sizes. Through these holes one looks at sheets of colored gelatine paper. The object of this apparatus is to experiment upon the relation of color to the apparent size of the surface. The apparatus was designed by Dr. Kirschmann, and is described and illustrated by him in Vol. VI. of the “Studien,” page 432. One large chronoscope used for demonstration purposes in the class room; it has a large ground glass dial, 40 centimeters in diameter and lighted from behind like the dial of a tower clock. Costs 400 marks. An apparatus for the demonstration of after images costs 48 marks. A similar apparatus for demonstration purposes by means of which one compares the endurance of the after images of sound and light, 60 marks. One model illustrating the movements of the eye, 45 marks. One model demonstrating the retinal images, 34 marks. One large “rotation-apparat,” a sort of colossal lecture room color mixer, costs with colored disks 175 marks. To this list must be added various keys, contacts, commutators, which are as a rule much better made in America. Any or all of the above apparatus can be secured from Wundt's mechanic, Herr Krille.

One of the most important pieces of apparatus, because of its utility, is the Baltze kymograph. It is absolutely indispensable—more so than the chronoscope—to a laboratory in experimental psychology. The late one is much improved over previous ones, and is shown pretty well in a cut on page 19 of Langendorff's new book, “Physiologische Graphik.”

Only two psychological laboratories in all Germany were equipped with this admirable piece of apparatus, the one at Leipzig and the one at Bonn. The following is a brief description:

A carefully turned hollow brass cylinder is set in rotation by means of a finely constructed piece of clock-work, which is contained in a brass box. This clock-work is regulated in its going by a pair of governing fans, after the principle of Foucault. A small lever serves to start and stop the clock-work. The clock-work turns a horizontal metal axis, to the end of which is attached a metal disk (about five inches in diameter), and which in rotating turns by means of friction the upright axis of the drum or cylinder already mentioned. By a simple and sure device the speed of the rotating drum can be accurately regulated. The drum can be made to turn in a horizontal as well as vertical position, and also has a little attachment whereby use can be made of a little electric motor or a heavy weight, if very great rapidity is desired. The drum of the new Baltze apparatus has a height of 13 centimeters, and is 50 centimeters in circumference. This drum is covered with a strip of smoked paper, on which are traced the curves. By a series of exchange wheels, in different combinations, the kymograph can be made to rotate the drum anywhere between 3 seconds and 90 minutes. By using a weight and tying down the governors (which can be done without injury), Tigerstedt succeeded in turning the drum at such a rate that every millimeter on the paper corresponded to .0016 seconds in time. The student in psychology at Leipzig has also the advantage of Ludwig's renowned "Physiological Institute." Baltze, who contrived the kymograph which we have attempted to describe, is Ludwig's mechanic. Wundt delivers lectures on history of philosophy, but no lectures on psychology, leaving the latter task to his assistant. Külpe, whose long laboratory training gives him a fund of material for illustrations along every line of experimental work. Glockner's lectures on pedagogical psychology, as well as those of Leydl, are exceedingly interesting and valuable. Ludwig's lectures on "Empfindung and Bewegung" are more *psychological* than physiological, and should be down on the schedule of every student who pretends to study psychology at Leipzig.

HALLE.

At Halle there is no laboratory at present in experimental psychology, though one is contemplated. The writer visited Halle to confer with Professor Uphues and Dr. Huffert, as well as to hear Benno Erdmann in his lectures.

JENA.

It was impracticable to include Jena in our "Rundreise," much to our regret. However, Dr. Ziehen of the medical faculty, lectures on physiological psychology, and his courses are very popular. His lectures have just been published in outline, and a helpful and suggestive book they make. He also lectures on brain-anatomy (privatim), and on psychiatry (publicee), both one hour a week courses. Professor Biederman in the practice courses in his "Physiological Institute" offers much of interest. He is, as his record as a lecturer and writer shows, most deeply interested in psychology in its modern garb.

BONN.

The psychological laboratory at this university has an interesting history indeed. Toward the end of the year 1887, Professor Lipps, now in Breslau, and Dr. Martius, the psychologist at Bonn, proposed to the philosophical faculty and university senate that appropriate rooms be set aside for the purposes of experimental psychology. Through a lecture on the "Aims and Results of Experimental Psychology,"[1] this endeavor became widely known. The existence of the present laboratory was, however, not the result of these overtures, which were rejected at the time.

The rooms in which the psychological investigations are at present made belong to the Physical Institute. They were generously placed at disposal by Professor H. Hertz, the famous electrician and successor to Professor Clausius. The psychological laboratory at Bonn must, for the present, be looked upon as a private undertaking; it does not enjoy the patronage of the state. The admission of the students occurs in the form of a "privatissimum."

The arrangements are in general copied after those of the Leipzig laboratory, nearly all the apparatus being duplicate of Wundt's own. The most important instruments are all at hand. One of the late acquisitions is a Baltze kymograph of the most excellent workmanship. There is also an improved form of the Angell apparatus for investigations along the line of sound-intensity. The rooms, four in number, are admirably adapted to the purpose for which they are used. Indeed there is no psychological laboratory in all Germany equally fortunate in this respect. The dark room is a little gem, and a model of its kind. For the rest we shall let Dr. Martius speak for himself, and append a quotation from an interesting letter, which was, however, not written primarily for publication.

"The participation of the students in the investigations is naturally small. There is lack of such as are inclined to devote themselves for a long time constantly to psychological investigations. Workers are always present, but the facilities exceed the demand.

"This brings up the question: To what extent should the students take up independent work in the psychological laboratory? The experimental method in psychology has achieved such brilliant success that a scientific treatment of psychology which is not based on this foundation has become at the present day inconceivable. The fundamental facts of mental life, the sensations and perceptions, can be investigated only in this way. Moreover, the truth that a thorough knowledge of the facts of consciousness is the proper foundation for the remaining philosophical disciplines is daily receiving wider recognition. Thus the progress of psychology and philosophy depend to no small degree upon the development of experimental psychology. Wundt once made the remark that he expected to live to see every university in Germany provided with a psychological laboratory. If a retrograde movement does not occur, this expectation will surely be soon realized. The state authorities in the very interests of their universities will not be able to withdraw from the responsibility of furnishing a solution of this problem.

"This, however, does not decide how far the independent psychological investigations in the laboratory are to be open to the student. It appears to the writer not to be in the interest of psychology itself for this to occur to too great an extent. What every student who is to be examined in psychology (and also every student of medicine) ought to know, can be presented in lectures, provided the proper apparatus and lecture-rooms are at hand. The psychological work itself, on account of its difficulty, should be reserved for a limited number, who have a special interest in and enthusiasm for the subject. The future academic instructor in psychology should have a thorough preliminary psychological training if he is to be anything more than a historian and interpreter of psychological systems. For the purposes of instruction, however, lectures well supported by demonstrations are sufficient. It is, moreover, to be expected that with such limited and modest demands the authorities will be more inclined to keep pace with the progress of the times."

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[1] "Ueber die Ziele u. Ergebnisse der Exper. Psychologie." Bonn, 1888; 5 pp.

Dr. Martius lectures twice a week (privatim) on Grundzüge der Psychologie (with demonstrations), and directs research work one hour each day (privatim and gratis). Professor Neuhauser lectures on general psychology four times a week. Professor Nussbaum of the medical faculty lectures on the "Anatomy of the Sense-organs," Dr. Koch on the "Physiology of the Sense-organs," the former two hours, and the latter once a week. The latter also lectures on hypnotism, sleep and narcosis. Professor Schaefhausen lectures on anthropology twice per week. Pflüger's "Seminar" is given over largely to subjects of great interest to the psychological as well as the physiological student. Meyer lectures four times a week in a charming manner upon history of pedagogy.

This concludes our description, which is of necessity "sketchy," and in a measure incomplete as to details. We shall take pleasure in answering inquiries of those wishing more detailed information.

UNCONSCIOUS SUGGESTION.

During the summer semester of 1891, I gave a course of lectures on hypnotism in the auditorium of the Burghölzli Asylum (Zürich) to the students of the medical faculty of the university. At one of the lectures the young and intelligent attendant K. from Würtemberg was used for purposes of demonstration. After a few suggestions he fell at once into a deep sleep. I then gave him various post-hypnotic suggestions of a hallucinatory character, which succeeded well. In a water-bottle he saw several gold-fish that were not there; he saw a suggested cat, felt of and stroked her, etc. I then suggested to him in hypnotic sleep that on waking he should feel a strong desire for defæcation and that he should ask me for permission to leave the room immediately. He was scarcely awake before he complained to me in a low voice and asked to be allowed to leave the room. I allowed him to go and thought no more about him. A few hours later the assistant physician told me that the attendant K. had been attacked with violent diarrhœa and vomiting, together with headache. He had attempted to suggest these symptoms way, but in vain. I had the attendant called to me, as it was evident that my suggestion had had a much stronger effect than I had intended—a thing that as good as never occurs with me. After K. had recounted the symptoms of his suggested cholera I hypnotized him again and declared briefly and decisively that all his symptoms were past and that in the future he would never again experience anything more than would be contained in my suggestions. On awaking he declared that he was perfectly well and departed. On the next day I asked him how he felt and he replied that he was perfectly well, with the exception of a slight headache. This astonished me, as I knew that headaches are very easily suggested away. I asked concerning the nature of these headaches and he answered me in these words: "I have had these headaches for two years (he had been only a few weeks in the asylum as attendant). At that time I had an inflammation of the lungs (pneumonia) and with it severe headaches. The physician said to me that these headaches would never leave me again, as they were an inheritance from my father. In fact I have never completely lost them since then. Sometimes they are more severe, sometimes weaker, but even when I am in the best condition I always have a dull feeling of pressure in the head."

This declaration of the young man opened my eyes. That he was very suggestible had been proven by the intense effect of the previous suggestion. It now seemed to me highly probable that his two year headache was nothing else than the result of an unconscious suggestion on the part of the physician who had treated him for his pneumonia. I questioned him in detail. He said that his father had suffered his lifelong with headaches, but that he himself had in his youth only temporary headaches. Only since the pneumonia, i. e., since the remark of the physician, had the headaches increased, and becoming completely localized, had never left him for a moment. I then declared to him categorically that the physician had at that time made a stupid remark, that the headaches were not an inheritance and could be easily removed and that I would now cure him completely. I hypnotized him again, laid my hands on both sides of his head, suggested a strong electric current (which was of course not present) and declared the headaches to be now completely and definitely past forever. When he awaked the headache was gone. And in fact this single hypnotizing was sufficient to remove the headaches, at least up to the present;—to be sure, only four months have passed. After exhaustive night-watches and the like he has a few times had temporary headaches, but the chronic trouble is over.

This case seems to me very instructive, because it shows how easily physicians without knowing it can produce sicknesses that are not present by means of pessimistic prophecies, by anxious looks or by making the patient anxious. These are diseases suggested by the physician; they are not imaginations of the patient, but can be really painful, obstinate and serious troubles. Just as suggestion can cure an existing sickness, so it can produce one that does not exist. It is a two-edged sword, but is dangerous only when used by people that do not understand it. For this very reason it is necessary that the subject should form part of a physician's training.

In earlier years I myself evidently committed the very same fault as the physician just mentioned. An attendant suffered, as on previous occasions, from disturbances of the digestions and said she had pains in the stomach. We made an investigation, palpated the stomach carefully, asked if she felt pains, to which she answered in the affirmative. We thought we had found the sore place, looked sober and ordered milk diet and strict rest in bed, for we thought we had found an ulceration of the stomach. The cardialgia and the painfulness for pressure on the particular place in question grew continually worse; the patient could scarcely retain the milk, became emaciated and lamented much. Nevertheless no hemorrhage followed and otherwise no objective symptom could be discovered. For several months we kept her in bed. Gradually she recovered; yet for years the stomach remained very sensitive and we really believed that an ulceration of the stomach had taken place. After I had at a later date, 1887, through Bernheim in Nancy, become acquainted with suggestion, I hypnotized this attendant also. She proved to be extremely suggestive; everything succeeded with her, even the most absurd and intensive effects. It was a light matter to produce complete health in her case. Since that time we have had no occasion to trouble ourselves about her health. In the two other institutions in which she has since been employed it has remained excellent. In my mind there is not the slightest doubt that her former long sickness, together with the emaciation and debility, was called forth by our anxious investigation and the strict regulations made through fear of a perforation of the stomach. She surely had no ulceration of the stomach, and if we had at that time been acquainted with suggestion we could have cured her digestive troubles, together with the cardialgia and pains from pressure, in a few days.

PROF. A. FOREL.

EYE TESTS ON CHILDREN.

Tests on the strength of sight were made in the public schools of Worcester, Mass., U. S. A., in connection with the physical measurements carried on last spring. In tabulating the results those cards showing defects of less than fifteen per cent. from the normal were thrown out, as it had been found that an almost imperceptible decrease in the amount of light had a comparatively great effect on the result and consequently these cards were within the limits of error. A cloudy day likewise caused the percentage to drop, as did also fatigue. In sorting the cards after this elimination, it was found that out of 793 boys 308 or 38.84%, and out of 602 girls 313 or 52% were short-sighted.

The cards were now arranged according to the school-grades. In the first two grades the percentage of defective eyes is lower for girls than for boys, but in the others the reverse is the case. On the other hand the results vary from grade to grade. Both boys and girls start with a low per cent., 35 for the boys and 31.4 for the girls. In the next two grades a great increase is observed, the figures being 52.7% and 67% for boys and girls respectively. In the fourth grade there is a drop to 38% and 48.9% respectively, in the fifth a slight increase to 41.6% and 51% and then a steady decline to the ninth grade, where it is 18% for the boys and 24% for the girls.

The importance of this fact lies in its bearing on the question of the influence of growth on the susceptibility to disease. This rapid decrease in the percentage of defective eyes corresponds in time with the acceleration of growth attendant on the period of adolescence. It has generally been supposed that this increased rapidity of growth is attended by an increased susceptibility to disease and injury, but the observations made by Dr. Axel Key in the Swedish schools seem to completely refute this idea. He found that at no time were children better able to withstand disease than at this period, while before and after it they were especially susceptible. The decrease in the percentage of defective eyes at this period may be accounted for on the same ground.

The cards were also sorted with respect to the amount of weakness of sight found in each sex and finally with respect to the amount of weakness displayed by each of the eyes. Among the boys 46.5% were between 0.62 and 0.85 of the normal strength, 24.2% were between 0.50 and 0.62 of the normal, while 30.25% were below this. Among the girls the percentages were 47.8%, 24.25% and 28% respectively. Among the boys both eyes were defective in 53.77%, the right eye in 19.6% and the left eye in 26.63% of the cases. For the girls the figures were 56.58% for both eyes, 20.55% for the right eye and 22.86% for the left eye. From this it would seem that the left eye in both sexes is more likely to be defective than the right eye and in boys more often than in girls. Among boys the defects seem to be more serious in a larger number of cases.

The instruments used in the tests were the Snellen test-types for the first five grades and the Galton eye-test for the other grades. The use of the former was necessary in the lower grades on account of the youth of the children. The variation between the results of the two instruments falls within the limit of error for fatigue or change of illumination and so has no influence on the results as reckoned above.

G. M. West, Ph. D.

REACTION.

Külpe und Kirschmann. *Ein neuer Apparat zur Controle zeitmessender Instrumente.* Beschrieben von O. Külpe und A. Kirschmann. Phil. Stud., 1892, VIII. 145-172.

Titchener. *Zur Chronometrie des Erkennungsactes.* Phil. Stud., 1892., VIII., 138-144.

Dr. Külpe and Dr. Kirschmann describe an instrument devised by Prof. Wundt to regulate the Hipp electric chronoscope. It is now recognized by psychologists (though apparently not yet discovered by physicists) that the chronoscope only gives correct times when the relative strength of the current and spring are so adjusted that the latent times in drawing up and releasing the armature are alike. The chronoscope is empirically regulated by measuring a standard interval of time, and adjusting the current and spring until the chronoscope gives this time correctly. Prof. Wundt has constructed a large falling hammer with a lever and weight, which give times up to 616σ. The time required for the hammer to fall cannot be calculated theoretically and can scarcely be measured with a tuning fork, but may be determined with a chronograph. According to the tests made by Külpe and Kirschmann the mean variation of the hammer as tested by the chronograph was on the average 1.04σ. This includes the variable error of the chronoscope, but not the constant error which would be carried over to the chronoscope. The variable error of the chronoscope and hammer combined was on the average 1.04σ, which may be regarded as exactly the same as before. The variable errors of the chronograph used at Leipzig and of the chronoscope are consequently alike, which is contrary to Wundt's statement: "Die Feinheit und Genauigkeit ist also hier (with the chronograph) eine reichlich zehmal so grosse als bei dem Hipp schen chronoskop." (Phys. Psy. II., 282.) Külpe and Kirschmann find the error of the chronoscope to be much greater when the hammer is placed in a secondary circuit. A secondary circuit is necessary in measuring reactions with the old form of the chronoscope, and this should consequently be discarded. The constant error of the chronoscope used at Leipzig was found to be over two and a half per cent of the time, and this correction should probably be made in researches from the Leipzig laboratory, in which this chronoscope has been used. The chronoscope may, however, be readily regulated so as to have an error less than one-fourth of one per cent. Külpe and Kirschmann find that the time the current is broken does not appreciably affect the latent time of magnetism, consequently if the chronoscope be regulated for a standard time, say 100σ, longer times will also be measured correctly. The old form of the chronoscope (in which the hands run while the circuit is broken) was used in these experiments, and they should be repeated with the newer and more convenient form, in which the hands may be made to run while the circuit is closed.

Mr. Titchener gives the results of experiments in which the distinction between muscular and sensory reactions was used to determine the time of perception. The paper is the first part of a research concerned with the time of association. It is maintained by Wundt that if in reacting an observer in one case direct his attention to the movement and in another to the stimulus, the difference in time will give the time required to perceive the stimulus. On this supposition Titchener obtained the following times (in thousandths of a second) for three observers.

Excess in time of sensory over muscular reactions.		
81.4	84.4	97.
Perception-time for a color.		
29.5	30.2	28.1
Perception-time for a letter.		
53.5	52.7	51.5
Perception-time for a short word.		
51.8	50.1	45.3

J. McK. Cattell.

Columbia College.

June 6, 1892.

DR. G. S. HALL:

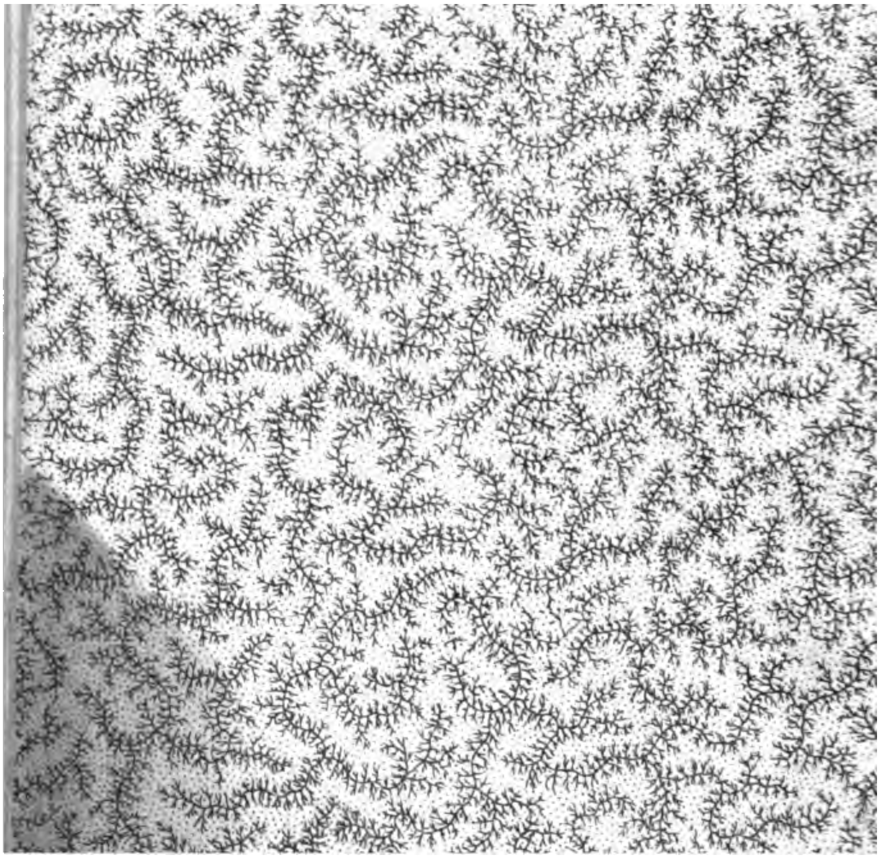
DEAR SIR:—I will gladly do my best to give you some account of the philosophical work at this university. The time, however, is not very propitious, for we stand in Scotland just now between the old and the new. The Universities' Commission, now sitting, has just framed ordinances (which come into operation next session), under which greater latitude is allowed to the ordinary student, i. e., the way of choosing his curriculum, and at the same time greater opportunity is offered for specializing in honours courses. We hope that this may lead to a considerable increase in the number of honours students, and a corresponding advance in the amount and the standard of higher teaching in the different departments. It will be necessary to this end that the university staff of teachers be increased; and from the increased funds at the disposal of the Commissioners we look for an improvement in this respect also. In philosophy, for example, the Faculty of Arts here has asked for a lecturer in physiological experimental psychology, and a lecturer on ancient philosophy.

These things, however, are still in the future, and I can only speak of the past. There are only two official teachers of philosophy in the university, the professor of logic and metaphysics and the professor of moral philosophy; though we have also a professor of political economy, a professor of the theory, history and art of education, and a professor of public law, all included in the philosophical department of the future. The lectures of the professor of education (Professor Laurie) at present embraces a large amount of psychological training, and the theological lectures of Professor Flint often diverge into philosophy; and these well-known names could not be omitted in any estimate of the philosophical work of the university. Philosophical teaching in the stricter sense, however, devolves entirely, as I have said, upon Professor Calderwood and myself. Every Scottish M. A. has hitherto been obliged to include "logic and metaphysics" and "moral philosophy" in his curriculum, and hence the energies of a Scottish professor have to be mainly devoted to the elementary training of the large masses of students which, in a university like this, pass annually through his hands. This leaves less time than is desirable for the development of honours teaching.

The ordinary course in "logic and metaphysics" embraces the elements of logic and empirical psychology, together with an introduction to the problems of epistemology and metaphysics, such as may be gained from a study of English philosophy in Locke, Berkeley and Hume. The character of Professor Calderwood's course in moral philosophy may be best gathered from his much-used "Handbook" of the subject. In connection with both chairs there has existed, for many years now, an advanced class for honours students. In "logic and metaphysics" this has been devoted hitherto entirely to metaphysics and the theory of knowledge in connection with the history of philosophic thought, especially of the Kantian and post-Kantian era. The corresponding class in "moral philosophy" embraces a survey, historical and critical, of ancient and modern ethics. Finally, it would be wrong not to mention that to each of the chairs is assigned a class assistant, who, in addition to assisting with the paper-drudgery of examinations and essays, gives a certain number of lectures, both to the ordinary and the Honours class. These assistantships are held by distinguished graduates, generally for a period of three years, and have formed in many cases the training-ground of future professors. It is our object by more adequate endowment of these positions, as well as by the institution of lectureships, to increase the range of philosophical teaching in the university, and so keep pace with the increasing demands of specialism within our subject.

Believe me, yours very faithfully,

ANDREW SETH.



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